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THE RECENT CHANGE OF ATTITUDE TOWARD THE LAW OF CAUSE AND EFFECT[1]

By Professor P. W. BRIDGMAN

HARVARD UNIVERSITY

NEARLY every educated person, brought up in present-day society and under the influence of the scientific ideas and spirit which pervade our intellectual life, prides himself in the belief that nothing happens without there being some cause for it. We may briefly characterize this attitude of ours by saying that we believe in the law of cause and effect or in the causality principle. To many of not too cynical a temperament this attitude will seem the most sweeping characteristic of the mental difference between the superstitious savage and the cultivated product of a hard-won civilization.

It is now becoming common knowledge that one of the most startling developments of the altogether surprising progress of physics in the last few years has been a weakening of the belief of the physicist, at least, in the validity of the causality principle. I want to examine with you this situation—to inquire in what sense we are losing our conviction of the validity of the causality principle, and to discover some of the implications. I want especially to emphasize that I am concerned only with the objective aspects of the situation. The idea of causality which we shall discuss is as remote as possible from the subjective questions of free will or determinism which are often associated with it, both in popular discussions and in a number of recent more technical discussions by scientific men. We shall be concerned only with the domain accessible to experiment, and the causality principle, in the sense in which I use the term, is a principle dealing with the findings of actual experiments.

[1] Address delivered at the University of Wisconsin, April 21, 1931.

We shall not be able to get very far without trying to make a little more precise what we mean by the causality principle. What do we mean in any concrete situation by saying that the event A is the cause of the event B? There is at least one obvious condition to be satisfied; B must occur later than A, for no one would think of saying that something happening now was the cause of something which had already occurred in the past. But mere sequence in time is not enough. For instance, in society as at present constructed one's education is almost always completed before embarking on the sea of matrimony. But do we therefore say that the cause of our getting married is that we are educated? That is, there may be sequences in time which we recognize as due to accidental and irrelevant associations. What more, then, is necessary than mere sequence in time? I believe that examination will show that we must at least have invariable sequence—the event B must always follow the event A under all sorts of conditions. But this involves being able to repeat the experiment, and it seems to me that the idea of invariable recurrence on repetition is indeed essentially involved in the notion of causality and perhaps comes closest to what is usually meant by causality. For example, suppose that I have a heavy weight attached to a support by a string; then I shall find that whenever I cut the string the weight falls, so that my definition in terms of invariable sequence would lead me to assert that the cause of the falling of the weight was the cutting of the string. But the situation is not quite as simple as this, for in the repetition of the experiment as ordinarily performed a number of the conditions are not varied, and who shall say that some one of these invariable conditions is not responsible, and therefore the true cause, of the weight falling? For instance, if I could by some heroic means remove the earth, then we are all convinced that the weight would no longer fall when the string is cut, and so we could justify the contention that the true cause of the falling of the weight was not the cutting of the string but the presence of the earth. But a still more critical analyst might contend that even the presence of the earth would not be sufficient to make the weight fall if it were not for the law of gravitation, and that therefore the true cause of the weight falling is the law of gravitation.

An impartial examination of the various arguments would convince us that there is much to be said for each of them; if we should decide that one of the claimants was correct and the others wrong, I think we should find it impossible to defend our decision against the objections of the losers. This sort of dilemma occurs very frequently. Sometimes one tries to save the day by the introduction of such ideas as that of immediate and remote or primary and secondary causes, but no such compromise ever works satisfactorily, as may be seen in our simple example by selecting some definite feature as the primary cause of the falling of the weight, to the exclusion of all others.

The conclusion to be drawn from these difficulties is, I think, that the notion of causality is not sharp and can not be made logically precise, but it is only a common-sense notion, used in describing many situations of daily life with sufficient accuracy for ordinary needs. I believe that analysis will show, however, that all situations which may be described from the common-sense point of view as those to which the law of cause and effect applies have certain more general properties. Whenever our knowledge of a situation has become sufficiently deep for us to attempt an analysis of its various events into cause and effect, we find that we have merely had sufficient experience with the situation to observe certain uniformities of behavior, certain regularities in the sequence of events, of the sort that when certain events occur other definite events follow. This characteristic of uniformity I take to be the fundamental feature in the situation. The analysis of events into sequences which are causally related is simply one of the ways of exhibiting certain uniformities and recurrences.

What now is the criterion that in a given physical system one has mastered the essential uniformities and has not merely stumbled on something accidental? I think that every one will admit that the supreme test is the ability to predict; if our neighbor is always able to accurately predict the future behavior of any physical system, I believe that we should all admit that he had completely mastered the uniformities of the system. Conversely, I think that we shall easily admit that without uniformities in the past behavior of a system no conceivable basis for predicting the future exists. The essential thing, however, is the ability to predict; granted this we have comparatively little concern with the language or system of philosophy in terms of which our neighbor may choose to describe his feelings in the matter. Furthermore, we shall all, I think, admit that in a system completely governed by the law of cause and effect the future should be predictable if we know the complete past history of the system, and conversely, if we know how to predict the future from the past, we should expect to be able to formulate some sort of statement as to the uniformities of the system which could be put into the form of a law of cause and effect.

It appears, then, that uniformity, causality and predictability all have certain common aspects, and

for rough purposes may be treated as more or less equivalent. It will suit the purposes of this exposition to lay the emphasis on predictability and I shall in the future be concerned with this aspect of the situation. We may now reformulate our statement of the beginning that nearly every civilized person believes in the law of cause and effect by saying that nearly every civilized person believes that the future of a system is in principle predictable when we know all about its past behavior. To avoid argument, I am perfectly willing to make the qualification that this discussion shall be limited to the realm of purely inanimate things, leaving out altogether biological phenomena.

As a historical fact, classical physics has been committed to a much more restricted formulation of the possibilities of prediction in stating that the future could be predicted if the present position and velocity of each particle is known. This conviction arose from the belief that the laws of Newtonian mechanics, which can be formulated in terms of differential equations of the second order, completely govern the motion of the actual physical universe. This formulation, however, is much more restricted than we need to make it for our argument and in fact there are difficulties with the Newtonian conception of predictability when attention is paid to various propagation effects which are discussed in elementary expositions of relativity theory. The essence of the situation for our purposes is that we have become convinced that the behavior of nature exhibits regularities of such a kind that from observation of the events of the past we can predict those of the future.

What now is the basis of this belief of ours in the thorough-going uniformity of nature or in the essential predictability of future events? It must at once be admitted that this belief in its wide-spread acceptance is largely an outgrowth of the last few hundred years. Savage and superstitious man sees in nature nothing but the capricious; it is only after long experience and observation that the simple uniformities begin to emerge, first the regularities in the motions of the heavenly bodies and then the same regularities in the simpler terrestrial mechanical systems. It was a tremendously stimulating discovery to find that simple uniformities in the motions of the planets which could be formulated in simple mechanical laws recurred also in the motion of terrestrial systems, and to find that as we acquired skill in analyzing systems of increasing complication these same simple uniformities continued to describe the uniformities being newly discovered. It is no wonder that after physics had experienced success after success in mastering systems of increasing complication it came to look on this success as no accident but the expression of an underlying principle. In some such way arose the conviction of the essential uniformity and predictability of nature; it was of course recognized that in the walks of daily life, as distinguished from the artificial situations of the laboratory, the power to predict was more conspicuous by its absence than its presence, but this was ascribed merely to the enormous complication of actual physical systems, and the conviction became general that by sufficiently refining the accuracy of our measurements and increasing their scope we might some day hope to reduce the most complicated system to predictability with any desired degree of accuracy.

It is to be emphasized that although the justification for this conviction arose entirely from experience of the external world, it constituted, nevertheless, even in the simplest possible case, an idealization of that experience. For in any concrete physical situation a prediction about a future event could be verified only within the limits of experimental error. The conviction that the future is predictable down to the last detail arose entirely from the experience that whenever the precision of measurements was increased the predictions which it was possible to make on the basis of the measurements always became better. But the uncertainty arising from experimental error could never be eliminated, and even in the most favorable cases the jump from the actual experiment to the ideal formulation of it involved a process which the mathematician or the physicist would describe as a long range extrapolation.

But now in the last few years all these expectations have changed, and the change has arisen primarily from the discovery of new experimental facts. Nothing previously found by experiment ceases to be true. Physics never has to retract statements about experimental facts when these statements are made with sufficient care to reproduce the physical situation with fidelity, that is, when due regard is paid to the limits of experimental error. Thus the law of gravitation should not be formulated baldly as the law of the inverse square, but rather that the attraction varies as the inverse square within certain limits of error, perhaps one part in a million, or whatever the greatest precision happens to be at present. The only genuine retractions which physics or other science has to make are in its statements about what it anticipates may in the future be found to be true about experimental realms not yet entered. So in the present situation, physics finds that it must retract a hope or expectation which it had based on previous experience, but it has not had to retract any statement about actual experiment. The expectation was that by increasing the accuracy of measurement indefinitely we would be able to make predictions about the future

with indefinite precision. This turns out not to be true, for we have recently found that when we increase the refinement of our measurements beyond a certain point and enter a domain of small things not before accessible, the new domain is full of the most capricious irregularities, unlike the regularities in the domain of ordinary experience, so that in the new domain no refinement of measurement enables us to predict the future. The new domain in which this disturbing state of affairs holds is the domain in which the motions of single electrons or atoms are concerned, and is, of course, enormously remote from the domain of everyday affairs, in fact, so remote that only within the last few years have physical methods been sufficiently refined to enable us to enter this domain at all. The situation is not unlike that presented by the semi-convergent series of the mathematician. Situations are not uncommon in mathematics in which the goodness of the answer to a problem may be improved up to a certain point by increasing the labor of computation, but if labor is put into the calculation beyond this point the answer becomes poorer instead of better.

Our new understanding of the experimental situation can be made in the following bald statement: "As a matter of fact, events are not predictable in the realm of small things." This is practically equivalent to saying that in the realm of small things the law of cause and effect does not operate.

Some little experience has proved to me that this bald statement is likely to awaken in many persons the most active hostility, and this audience would indeed be unique if it did not contain an appreciable number of persons who positively bristle with animosity at such a statement. The reaction which this statement is most likely to produce is this: "You have not proved that in the realm of small things the future is not predictable, but all you are justified in saying is that you have not yet found how to predict the future. In fact, judging by past experience, there is every reason to think that if we keep on trying we shall eventually discover how to predict in this domain which at present seems so hopeless." This objection, I am sure, will appeal to many as entirely sound, but I believe that nevertheless it can not be maintained, and one of the points which I am most anxious to make this afternoon is my reason for thinking this position not to be sound. I hope that the positiveness of the assurance of the physicist in this matter will not give the unpleasant impression of mental arrogance which it easily might. I believe that every physicist recognizes that one can never say with complete assurance that his present theory is correct. There is nevertheless at least one statement which, when it can be made at all, can be made with absolute assurance, and that is that we are now taking into consideration ideas which had not previously occurred to us. All that the physicist is maintaining is that we have now at our command new experimental facts and new ideas and that in the light of them our former ideas must be modified.

It must be conceded that certain parts of the objection of our bristly critic are well taken. We have not, of course, proved that the future is not predictable, and we can say only that we are not at present able to predict and do not believe that we shall ever be able to. In fact, in the very nature of logic, it can never be proved that an entirely unexpected discovery may not be made some day which will enable us to predict the apparently unpredictable. We shall have to admit that from this point of view the tactics of the objection is very clever. From another point of view also the tactics is clever in that it puts the burden of proof on the advocate of the new point of view in effectively asking him "by what right do you expect that no one ever will find out how to do what to-day we do not know how to do?" I believe that nevertheless, in spite of the superficial strength of the objection, it is fallacious.

Let us in the first place examine what the experimental situation is which leads us to say that events in the realm of small things are unpredictable. When, in the domain of large things, we fail to predict, it is usually because of the extreme complication of the physical system, as, for example, in our endeavors to predict the weather or mob psychology. In the domain of small things, however, the element of complication is lacking and our failure arises from another reason. The reason we fail is because those regularities which are the basis on which we are able to make predictions in the simpler situations of large scale experience are entirely lacking in the domain of small things. Let us imagine the simplest possible large scale situation—a billiard ball rolling without friction or other interference on a table top. Let us imagine the table top marked with lines a foot apart. Then we all know that if we observe the ball to be at the zero mark when the second hand of our watch points to zero, and to be at the one foot mark when the second hand points to one second, when the second hand points to two seconds we shall find the ball at the two-foot mark. This is the simplest example well conceivable in which we project into the future a uniformity of behavior in the past, and, parenthetically, is doubtless the origin of the ordinary concept of velocity. But experiment shows that if we were dealing with an electron instead of a billiard ball the experiment would entirely fail, and if the electron had been observed at zero at zero seconds and at one foot at one second, at two seconds we might sometimes

find it at seven feet and sometimes at five feet, or sometimes at minus one foot, and indeed sometimes at two feet, like the billiard ball. As a matter of fact this ideally simple experiment probably has not been performed, but inference from the results in less simple cases leads us to be convinced that such would be found to be the state of affairs if the experiment were made.

If capricious results like those just described for the electron happened in the experiment with the billiard ball we would almost certainly say that the initial conditions had not *all* been the same the time the ball appeared at the seven foot mark as when it appeared at five feet, and we should endeavor to find something that we had failed to take proper account of in specifying the initial conditions. Furthermore, we are convinced that this procedure would be successful in the case of the billiard ball, and that search would disclose the missing feature. We are also convinced that it would need some rather striking feature to account for the billiard ball sometimes turning up at seven feet and sometimes at five feet. But in the case of the electron, all our experience indicates that the missing feature does not exist, but that in systems in which the initial conditions are completely identical the electron will sometimes appear in one place and sometimes in another. Anything more unlike ordinary experience would be difficult to conceive, and the consequence is that there is very little basis indeed for making a successful prediction of the future position of an individual electron.

The experimental evidence, then, apparently forces us to the conclusion that if some basis for predicting the behavior of a single electron is to be found, it must be entirely different from the basis of prediction for ordinary events. Why is it that the majority of physicists at present believe that there is good reason to think that this basis for prediction will never be found, but that on the contrary we shall always have to treat the motions of single electrons as beyond the reach of prediction, that is, beyond the law of cause and effect? The reason is not that the physicist is either lazy or a quitter. Part of the reason is rather to be found in the quite surprising success achieved within the last few years by that body of physical theory variously described as "quantum theory" or "wave mechanics." The very foundations of this theory contain as an integral part the hypothesis that the individual electrons, as also the indivisible units of radiation, have the fundamental property that in any specific situation their behavior as individuals can not be predicted, but only the average behavior of large numbers. One explicit deduction from the theory which is directly concerned with predictability has been much discussed, namely the Heisenberg Principle of Uncertainty. This is usually formulated as a statement about the accuracy of measurement, the fundamental idea being that if we strive to increase the accuracy with which we make one kind of measurement we must pay a price in a necessary decrease in the accuracy of some other kind of measurement. Specifically, I can not measure the position and the simultaneous velocity of the electron with any desired accuracy, but if I increase the accuracy of my measurement of position, my measurement of velocity becomes less accurate in such a way that the probable value of the product of the two inaccuracies is of the order of magnitude of Planck's constant, h, divided by the mass of the electron. The principle applies equally well to the measurement of the position and velocity of an ordinary body. The reason why the principle is important for the electron and not for ordinary bodies is that the mass of the electron is so very much less than that of any ordinary body that when I divide Planck's constant, h, by the mass of the electron I get a comparatively large number, that is, a comparatively large uncertainty, whereas the quotient of h by the mass of an ordinary body is so much smaller as to represent an uncertainty below ordinary methods of detection.

The Heisenberg principle, as I have just formulated it, does not seem to make statements about predictability, but that it really does may be seen by considering the significance of the velocity about which we are talking. To make the problem concrete, go back to the moving electron. If I observe it at the one-foot mark at a certain time and then one second later at the two-foot mark, I know that its velocity during this second was exactly one foot per second. It is not this velocity with which the Heisenberg principle is concerned, and indeed the Heisenberg principle sets no limit to the accuracy with which this velocity may be determined. The velocity with which the Heisenberg principle is concerned is the velocity to be ascribed to the electron after, not before, the second observation. Now the only way I have of checking whether any statement about this second velocity is correct is to predict where I shall find the electron at a later instant of time. If the Heisenberg principle is correct, this prediction can not be made with precision; we thus see that the Heisenberg principle is really a statement about the impossibility of accurately predicting the course of the electron.

There is a point here which it will pay to emphasize because there has been considerable misconception about it. The *modus operandi* by which the uncertainty gets into the situation is through the act of observation—the electron can not be observed without bouncing an atom of radiation from it or doing some-

thing equivalent, and whatever the process of observation, the motion is interfered with. The essential fact is not that the act of observation interferes with the motion; if this were the only effect we could allow for the amount of interference by calculation. The essential fact is that the act of observation interferes with the motion by an unpredictable and incalculable amount. The fact that the amount of interference is unpredictable is an integral part of the theory.

Those persons who for one reason or another are anxious to save the face of the causality principle have often stated the conclusion from the Heisenberg principle in another way. They say that the causality principle is still valid, only it turns out that we are unable to make the measurements which are demanded in applying the causality principle, that is, in making a prediction. This contention it seems to me may easily degenerate into a mere matter of words, and become highly unprofitable. The essential fact is that it appears to be due to a law of nature and not to any temporary failure of ours that we can not make the measurements that we demand for our attempted predictions. In this situation it seems to me that we are keeping as close as possible to the actual facts in making the bald statement that experiment now makes it highly improbable that the future is predictable.

Not only is the Heisenberg principle checked by experiment, but apparently all other deductions from the wave mechanics theory are also checked with equal success. In fact, the success of the theory has been so great that the statement is often made by its enthusiastic advocates that in no case where it has been correctly applied, that is, without blunders in the calculation, has it failed, and that no experimental facts are known which are in contradiction with it. The average physicist now takes the next step, and draws the conclusion that because of the success of the theory the fundamental hypotheses on which it rests must also be very probably correct. It must be admitted that this last step is rather dubious from the logical point of view, because it does not follow at all that because our conclusions are correct our reasoning or our premises must be correct, and in fact there are a number of instances in physics in which the fundamental hypotheses have been changed without changing at all the superstructure, as shown, for example, by the change in attitude toward the physical reality of the ether.

I think we would have to admit that if the sole argument of the physicist were the success of the theory as at present formulated we would have some ground for scepticism as to how long the present attitude would last. But the physicist has other reasons for his attitude. Along with his experimental activity he has been active in critically examining the fundamental concepts of physics. Among other things he has examined the grounds on which rest our conviction that nature is uniform or predictable and has come to the conclusion that at least the burden of proof is now on the side of those who maintain that nature is uniform and can be described in terms of a causality principle. The reasons for this conclusion I have already intimated, but they are worth repeating. The physicist recognizes that belief in uniformity or predictability is a belief compelled by no inner necessity, but is a belief that has gradually grown up as a generalization from large scale experiences; that as long as experience was confined to the large scale things of daily life an ever increasing number of phenomena could be brought under the approximate sway of the principle, but that as soon as physical methods became sufficiently refined so that we could deal with small scale phenomena, uniformities became less and less conspicuous, until we finally arrived at electrons and photons, the ultimate structural elements of the physical world as we know it, where we would expect the utmost in the way of simplicity and uniformity but where, on the contrary, experiment shows that uniformity in its original sense has entirely vanished.

The situation thus contains two elements: There is in the first place the recognition that the notion of causality, in the sense in which I am using it, was an outgrowth of experience, and that the extent to which the causality principle is valid is solely for experiment to decide. This attitude I believe must now be accepted by every one who will take the pains to examine the argument. In the second place there is the conclusion from experiment that as a matter of fact nature is very far from predictable or that the causality principle fails by large amounts to be valid for small-scale events. If the first point is accepted, then the second may be accepted as a summary of the best experimental knowledge at present, without resentment or antagonism or rebelliousness. This willingness to accept the findings of experiment must remain permanently a part of our attitude, whether or not future experiment justifies present optimism about the complete adequacy of wave mechanics. This, then, is the chief idea that I hope you will carry away from this talk; that it is purely a matter for experiment to decide whether nature is predictable in the domain of small things or not; that until some at present totally unlooked-for development turns up to prove predictability or to make it plausible, we must assume that the causality principle does not apply in this domain, and that this conclusion is to be accepted without prejudice or passion just as any other experimental result is accepted.

What difference is the recognition of this situation now going to make to us? As far as actual action in most concrete situations of daily life go, it will make practically no difference at all. Large-scale phenomena will remain for all practical purposes just as predictable as they ever were. The reason for this is that in no case have we ever been able to predict large-scale phenomena with more than a certain degree of approximation; the goodness of the approximation has been fixed by the accuracy of the measurements. Furthermore, in nearly all cases, the inaccuracies of our measurements arise from the ordinary imperfections of our instruments, recognized and well understood long before the Heisenberg principle was formulated, and these inaccuracies are so great that the uncertainty in our predictions arising from them is much greater than the uncertainty arising from the action of the Heisenberg principle. In most practical situations the Heisenberg uncertainty is so very far beyond the reach of detection by ordinary means that from the practical point of view its effects will in nearly all situations remain forever of purely academic interest.

There is a difficult point here which we may stop to examine for a moment. At first sight it is not easy to see how it is that if large-scale phenomena are built up from small-scale phenomena and if the small-scale phenomena are essentially unpredictable, the large-scale phenomena acquire approximate predictability. The reason is that although any single small-scale phenomenon is unpredictable, experiment shows that nevertheless there is a sort of regularity in large numbers of them which permits combinations of them to be predicted, approximately. This sort of regularity is of the sort that may be described as statistical. Let us go back to the illustration of the electron moving over the marks on a table, and let us suppose that the electron has been observed at the zero mark at the zero of time and at the one-foot mark at one second. Then it is a matter of experiment that if I attempt to predict where the electron will be found at two seconds I shall make a great many mistakes, but it is also a matter of experiment, neglecting a consideration which is not important for this argument, that if I always guess that it will be at the two-foot mark, I shall in the long run make fewer mistakes and obtain a better score than if I make any other guess. There is thus a certain regularity in the aggregate results of a great many experiments, although the results of individual experiments may fluctuate widely, being now greater than the average and now less. If I could make a great many experiments simultaneously, it is evident that I could make a good prediction about the average result of all of them taken together, because in a great many experiments those individual results which are too high will cancel with those which are low, leaving outstanding merely the average. Something very much like this is involved when experiments are made on ordinary matter, for even the smallest bits of matter that can be distinctly seen in the microscope still contain a very large number of electrons, and the behavior of the whole bit of matter is merely the average behavior of all its electrons, which can therefore be predicted with much success. In fact, as already stated, the mean fluctuations arising from the uncompensated fluctuations of the individual electrons are less than the uncertainties arising from other and more ordinary sorts of imprecision of measurement.

It might seem, therefore, as though there could never be any practical effects arising from such small-scale uncertainties. This, however, would be too hasty a conclusion. If one makes the deliberate attempt, it is possible to magnify such small-scale events sufficiently to bring them into the range of everyday experience. An example of this sort of thing is known to every physicist in the Geiger counter. This apparatus is so constructed that the effect of the entrance of even a single electron into the sensitive part of the apparatus is amplified with vacuum tubes to such an extent as to give a crack of sound in a loud speaker, or to perform other functions, such, for example, as starting or stopping a piece of machinery. Now the electron which enters the apparatus may be the result, for example, of the radio-active disintegration of a single atom. The disintegration of such an atom is the sort of thing that experiment and theory both show is essentially unpredictable. It would therefore be possible, by utilizing an arrangement of this kind, to make all the lights of a great city flicker up and down, and it would be absolutely impossible to predict when the next eclipse would occur.

Some of you may have read a recent story by Lord Dunsany in which a crazy power magnate wards off the vengeance of the powers above by gigantic prayer wheels driven by 10,000 horse power steam turbines. We may similarly romance about the future religion of a superstitious race by imagining in the inmost shrine of their temple a speck of radio-active salt in process of disintegration, and attached to this a train of vacuum tube amplifiers, which shall ever and anon flood the temple with light, or beat a tom-tom, or perhaps sacrifice a victim. A rather good argument might be made for this sort of thing, and it really appeals to the imagination in many ways, for we have here the possibility of a spectacular projection into the realm of ordinary sense of the eternally inscrutable foundations of our physical world.

In the realm of ordinary physical objects, however, this sort of unpredictability probably seldom occurs

unless it is the result of deliberate design. It is not quite so evident, however, what the true state of affairs is in biological systems. I gather the impression that at the present time a number of biologists are prepared to admit that not infrequently the adjustment of a single cell may be so delicate as to be thrown out of balance and a reaction started by the entrance of a single free electron or light corpuscle into the cell. In such cases the behavior of the individual cell must be admitted to be unpredictable. In one simple field of biological experiment the facts have already been definitely established. It has been shown that when certain unicellular organisms are radiated with alpha particles, the death of the organism results if a single alpha particle makes a direct hit on the nucleus of the cell. The death of any single organism is therefore an absolutely unpredictable event, although in a large colony of such organisms the average number of deaths may be predicted by the methods of life insurance statistics. The important question now is whether in large-scale organisms consisting of many cells the adjustment is ever so delicate as to be thrown one way or the other by the unpredictable action of only a few cells, or whether so large a number of cells must cooperate in any large-scale movement as to make all large-scale movements approximately predictable by the methods of statistics. This question can be answered only by experiment, and it is certainly one of the most important questions to which biology can address itself. At the same time it is obviously one of enormous experimental difficulty, for no experiment can be repeated under identical conditions on an organism complicated enough to have memory.

A word of warning may be interjected here. Many will be tempted to see a connection between the question of the predictability of the behavior of organic systems and those questions which have always exercised the human race, determinism and free will. It seems to me that there is no connection. The former is primarily a question of physical fact, while the latter are predominantly questions of a subjective character which involve those emotional experiences which the subject goes through when on the point of making a decision.

The concrete physical changes which are likely to arise from our modified attitude toward the causality principle are therefore small. I believe, however, that there will be very important effects on our methods and habits of thought and our entire outlook. One of the most obvious effects of these discoveries is in prescribing the program for future scientific investigation. One such possibility, I have just indicated, namely, the examination of the question as to whether the behavior of complex biological organisms is ever initiated by unpredictable small-scale events. A similar question arises with respect to other sorts of physical happenings; are there anywhere in nature mechanical systems of such a high degree of instability, as for example in the turbulent motion of a liquid, that an unpredictable small-scale event initiates a large-scale event, which thereby itself becomes unpredictable. Another possible program for future scientific investigation is in devising simple experiments which shall demonstrate less indirectly than we now can some of the statistical effects of small-scale events. For example, the invention of a photographic plate such that the impact of a single photon is sufficient to make a single grain of the plate developable would be an enormous assistance.

Apart from these concrete effects on our scientific program there will be many other less tangible results of our changed mental outlook, which I have not time to elaborate in detail here, but I shall try to give a few brief indications of what may be expected. A parallel may be drawn from the theory of relativity. Although the theory of relativity deals with phenomena which are so difficult to detect as to require instruments of the highest delicacy, nevertheless it has made exceedingly important changes in our attitude toward the fundamental concepts of space and time. I believe that in the same way the clear recognition that causality can not function in detail in small things, as has been supposed, but can have only a statistical meaning, must have important repercussions on our thinking. In fact, activity is already beginning in the ranks of the philosophers which bears out this contention. Many articles and even books have already appeared in which the endeavor is made either to explain away the importance of the new findings, or else to discover how we may adjust ourselves to them. The bearings on epistemology are apparently considered especially important. A single possibility may be mentioned. At a recent discussion between philosophers I heard it argued that the principle of sufficient reason is an absolute *sine qua non* of thought, that the human reason must by its very nature refuse to accept the possibility, to go back to the well-worn example of the billiard ball and the electron, that in one experiment the electron should appear at the seven-foot mark and in another at the five-foot mark, without there being some reason for the difference. To which I am afraid that the brutal physicist would be tempted to rejoin that if the human reason is incapable of accepting the situation, so much the worse for the human reason. However, these are questions of technical philosophy which are entirely beyond my sphere, and I shall not say anything more about them for fear of making even more egregious blunders than I may have already.

Apart from philosophical questions, however, it seems to me the realization that it is possible to exemplify on a large scale such things as our capriciously disintegrating radio-active material, which may serve as the nucleus of a superstitious religion, or may equally well serve as a most excellent gambling device, can not fail to get under the skin of the man in the street. This objectifies in the most striking way the limitations of the human intellect, and I believe that the greatest changes in our mental outlook will come as a consequence of the realization of just these human limitations—we had thought the human reason capable of conquering all things, we now find it subject to very definite limitations. We can definitely conjure up physical situations in which the human reason is powerless to satisfy itself, but must passively be content to accept phenomena as they occur, which constitutes in fact a reversion to the mental attitude of primitive man, which is purely receptive. What is more, the strictly scientific attitude recognizes no escape from the situation, but it must be accepted as inherent in the nature of things, and no way out attempted by such inventions, material or conceptual, as primitive man makes.

The realization of human mental limitations will, I believe, have the greatest effect, and the process of adjustment will be slowest, in such non-scientific activities as philosophy, religion, as already suggested, and very probably education, for some just apprehension of the possibilities of the human intellect should be imparted in any satisfactory educational program. The adjustment in scientific activity I believe will be made more rapidly, and in fact it is possible to see even now in what the adjustment will consist.

A formulation of the purpose of scientific activity which appeals to me as rather exhaustive is the understanding, prediction and control of events. It might be thought that the discovery that there are aspects of nature which are not understandable, predictable or controllable would work havoc with this scientific program. But the way out is already obvious. If it is true that there are certain aspects of nature which are neither controllable or predictable, then the obvious course is to avoid these aspects of nature. This may sound like a flippant suggestion, but the matter is really to a large extent in our own power. We have seen that although single small-scale events are unpredictable, the statistical average of large numbers of them is highly regular and predictable. The obvious course of action, then, whenever we want to be sure of the result, is to so arrange the apparatus or machine as to respond only to statistical averages, and not to function like a Geiger counter in response to single small-scale happenings. If it should prove that the large-scale behavior of biological organisms is unpredictable, then we shall take pains never to depend on the behavior of a single such organism whenever we have to be sure of our results, but this is hardly more than we do already.

The situation with regard to *understanding* small-scale events will probably take a little more adjustment, because it involves giving up an ideal which we had set ourselves. But even here the adjustment can hardly take more than one generation, and in science generations are short. Analysis will show, I believe, that what we call understanding consists in picking out from a situation elements with which we are already familiar. The difficulty in the present situation is that we are not familiar with systems in which individual events occur with no close connection with past events, so that naturally we are confused and seek for a hidden connection. But as our familiarity increases and the strangeness gradually wears off, we shall come to feel that it is natural and proper that small-scale events show only statistical regularities, and we shall come to be satisfied with our understanding of a situation when we have analyzed it sufficiently to show, if we are dealing with large numbers, the statistical regularities to be expected, or, if we are dealing with small numbers, the corresponding capricious variations. In fact, a number of the younger generation have already achieved this degree of emancipation, and the rest of us, by deliberate effort, may hope to attain it.

PALEONTOLOGY VERSUS DEVRIESIANISM AND GENETICS IN THE FACTORS OF THE EVOLUTION PROBLEM[1]

By Dr. HENRY FAIRFIELD OSBORN
AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK CITY

THIS is a study in what may be called *progressive or adaptive heredity;* it is the second of a series of communications I plan to give to the National Academy on the factors of evolution; as implied in the title, this study is directly opposed to all accidental, discontinuous or mutational hypotheses of the causes of the bio-mechanical evolution of the germ plasm.

Last year I summarized results obtained through

[1] Address before the National Academy of Sciences, Washington, April 28, 1931.

twenty years of research on the evolution of the Titanotheres, a family which evolved during an estimated period of twelve million years, from an animal about a foot in height to the gigantic "thunder-beast" or Brontothere stage. This year I add results obtained from twenty-one years of research on the evolution of the Proboscideans, namely, the mastodonts and elephants, an order which may be traced over twenty million years, from present estimates of Tertiary time, beginning with animals about two feet in height and rising to animals over thirteen feet in height.

Dynamically the Proboscideans are very strong where the Titanotheres are weak, namely, in the evolution of the grinding teeth and of the pair of superior incisive tusks around which the entire life history of these animals centers.

As previously pointed out we can not comprehend the hereditary germ plasm without intensive research on the bio-mechanical evolution of form concurrent with research and experiment in biophysics and biochemistry. Bio-mechanical adaptation has this advantage, that it can be weighed and measured in fossil bones and teeth over periods of millions of years. We can also clearly distinguish between organs and organisms which are *static* and those which are *dynamic* or in the state of movement. One part of an organ may be in rapid evolution while a contiguous part may be standing absolutely still, as brilliantly illustrated in the discovery of the shovel-tusker mastodonts in Central Asia and in Nebraska. The detection of these relatively static or dynamic conditions, of arrest or of motion, of acceleration or retardation, is denied to all biophysical and biochemical experimentalists working on living organisms. For example, the fruit fly, *Drosophila,* is probably a completely static organism. The same we know to be true of members of the mouse family, which stopped evolving at least a million years ago; it is probably true of the guinea pig family.

Physical or chemical disturbance of the inconceivably delicate and minute hereditary mechanism produces anomalies and sports which may be hereditary but can not set in evolutionary motion either an organ or an organism.

So far as we can see in paleontology, continued hard work along the lines of maximum resistance is essential to setting an organ in evolutionary motion or acceleration. Parts of organs which do the hardest work evolve most rapidly, because the universal distinction of the bio-mechanism in contrast to the inorganic mechanism is that it develops both in the individual and in the race in the line of maximum resistance, for example, the upper and lower grinders of the elephant. Under this condition it is most dynamic; it becomes static when the resistance diminishes; it retrogresses when the resistance disappears.

This contrast between static and dynamic organisms and organs extends even into the most minute subdivisions of organs as in the microscope composition of the enamel of the gigantic tusks and grinding teeth of the Proboscideans, all the rest of the animal (excepting the proboscis) remaining *static.* The most refined analysis of the tusk enamel by polarized light reveals this universal principle of adaptive reaction to lines of maximum resistance.

The modes of bio-mechanical evolution were first perceived by the Greek anatomists who discovered seven principles. Shortly before the time of Darwin two more principles were added through researches in embryology; altogether nine bio-mechanical principles were known to Charles Darwin. A summary of the fourteen factors at present known is as follows:

BIO-MECHANICAL PRINCIPLES

A. *Discovered through Human and Comparative Anatomy, Palaeomorphs*

Known to Lamarck, Darwin and Spencer

1. Bio-mechanical progression, hypertrophy
2. Bio-mechanical retrogression, atrophy
3. Bio-mechanical compensation, metatrophy
4. Bio-mechanical economy, eutrophy
5. Bio-mechanical change of proportion, allometry, metatrophy
6. Bio-mechanical co-adaptation, coordination, correlation, through interaction
7. Bio-mechanical auto-adaptation, through principles 1–6.

B. *Discovered through Embryology*

Known to Lamarck, Darwin and Spencer

8. Bio-mechanical acceleration of organs into earlier growth stages
9. Bio-mechanical retardation of organs into later growth stages

C. *Discovered through Paleontology, Neomorphs*

Unknown to Lamarck, Darwin and Spencer

10. Bio-mechanical germinal rectigradation, in the origin of new structures and organs
11. Bio-mechanical germinal pre-determination, in the origin of new structures and organs
12. Bio-mechanical phylogenetic acceleration, in the evolution of new structures and organs
13. Bio-mechanical phylogenetic retardation, in the evolution of new structures and organs
14. Bio-mechanical germinal potentiality, in the origin of new structures and organs

The remaining five bio-mechanical principles have been discovered through paleontology since Darwin's time. Neither Darwin nor Huxley had the least knowledge of the bio-mechanical factors of evolution as they have been revealed in paleontology.

CONCLUSIONS AS TO THE ORIGIN OF SPECIES

These fourteen bio-mechanical principles not only lend no support to the Darwin-DeVries hypothesis of mutation but are directly contrary to current assumptions and hypotheses by geneticists and experi-

mentalists that bio-mechanical adaptation can be induced by sudden changes in the genes. Whatever may be true of sudden bio-chemical adaptations a presumption from this overwhelming bio-mechanical evidence is that *both bio-chemical and bio-physical evolution is also along continuously adaptive and creative lines.*

William Bateson, founder of the genetic school, finally declared that genetics could not explain the origin of a single species. Modern paleontological research, on the other hand, reveals with absolute clearness and fullness not only how species originate bio-mechanically, but the inner interpretation of divergence in every grade of animal organization from the minute ascending mutations up to the higher points in the development of genera, families and orders.

The modes of these transmutations of form which from the same original materials produce the horse, the rhinoceros, the titanothere and the elephant are now absolutely clear, but the internal causes of these marvelous bio-mechanical transmutations of the germ plasm are far more mysterious and incomprehensible than they were in the time of Charles Darwin.

OBITUARY

ALBERT A. MICHELSON

AT one o'clock on Saturday afternoon, May 9, 1931, death came very quietly in his home in Pasadena to the most illustrious of the American physicists of our generation, at the age of seventy-eight years and five months. Six weeks earlier he had taken to his bed, after having made with his associates, Messrs. Pease and Pearson, enough observations to assure himself that his last experiment on the speed of light as measured in an evacuated pipe a mile long and three feet in diameter buried in the earth on the Irvine ranch near Santa Ana, California, was going to yield results as satisfactory as he had anticipated. This experiment had been planned for the sake of obtaining a check by a method entirely free from atmospheric effects of all kinds upon the accuracy of his next preceding determination made over a twenty-one mile stretch between California mountain peaks. He did not expect by these new experiments to exceed the accuracy previously obtained, but rather to add something to the *reliability* of the previous determination.

More than a month before his death, Mr. Michelson had known that he would never get up again, for a creeping paralysis was coming over him of which he himself was altogether conscious. His mind was quite clear until two days before the end, when a lesion occurred which brought on unconsciousness within an hour, an unconsciousness from which he never again awoke.

As one of the men who has had the most enduring and most intimate association with Mr. Michelson and his work, I esteem it a privilege to now make a few additions to my former appreciation of him and his achievement.

Under the caption, "Michelson's Economic Value," now published as Chapter VII in a volume by Scribner's entitled "Science and the New Civilization," I have attempted to appraise in broad lines the significance for our times of measurements of the highest skill and accuracy of the sort which Michelson has done and for which his name stands the world over, and I should like to refer to that appraisal and merely supplement it here by adding some details both of a scientific and of a personal sort.

Practically all Mr. Michelson's work in physics centered about determinations for increasing the precision of measurement. He has been called an extremely skilful and intelligent instrument designer, but while he was that he was much more than that, for his attention was always on the problem to be solved, not primarily on the instrument for solving it, and he was always seeking for problems incapable of solution save by improvements in the accuracy of measurement. Ten different times in the fifty-one years of his activity, extending from 1880, when at the age of twenty-eight he became the best known American physicist by virtue of his new speed-of-light measurement, up to 1931, when he died, still trying to prove the certainty of his determination and precision of that most fundamental constant, he made major outstanding advances, which I list as follows:

(1) Measurements of the speed of light, 1880–1931.

(2) Development of the Michelson interferometer, 1882, *et seq.*

(3) Ether-drift experiments, 1887–1928.

(4) The first analysis of the fine structure of spectral lines, 1894–1900.

(5) Development of the Michelson-Stratton harmonic analyzer, 1897.

(6) Development of the principle of the Echelon spectrograph, 1898.

(7) Perfection and increase in resolution of the line grating, 1902–1917.

(8) First accurate measurement of the rigidity of the earth, 1916.

(9) Development of the U. S. naval range finder, 1918.

(10) Direct interferometer measurement of the diameter of stars, 1921.

Of these ten, four—namely, the second, third, fourth and tenth—have to do primarily with the interferometer or its application to various sensitive measurements. It was in measurements relating to the interference of light and speed of light that he was by far the greatest expert that the world has yet seen. He spent his scientific life largely in these two fields. He was not an omnivorous reader of the literature of physics, and did not try to follow closely the developments of the theoretical fields of electronics and quantum theory. He was essentially a classical physicist, but any one who ever heard him conduct a Ph.D. examination in physics, and any one who ever attended his lectures or heard him speak knows that his grasp of classical physics was penetrating and precise. His lectures and his papers were masterpieces of elegance and conciseness. He used few words, but they were just the ones he wanted. Indeed, the precision of his mind was its dominating characteristic, which showed even in his sports. I have played tennis with him all my life, and his calling of balls, for example, was never generous either to himself or to his opponent. It was simply exact and just. Closely allied to this characteristic was his altogether extraordinary honesty. Pretense of any kind was utterly foreign to his make-up. Indeed, he was one of those very rare persons who would not even tolerate fooling himself with respect to his own motives, as so many of us do. If his conduct was ever ungenerous he knew it and frankly admitted it, whether he thought it wise to change it or not. Before I became intimately associated with him I had heard that he was considered by his pupils to be somewhat unapproachable, occasionally arbitrary, and at times dictatorial, if not unreasonable, but in the twenty-five years in which we worked together I could not have been treated with greater courtesy and consideration, even in the few cases in which we differed in judgment. His dignity and courtesy of bearing were altogether striking characteristics, and as the years passed he grew to be a man of great mellowness, kindness and affability.

Like many a scientist, Mr. Michelson was also an artist, with a keen feeling for form and color, as well as for music. He painted well, played the violin well, and did well at tennis, chess and billiards.

American science and the American nation have lost in his death one of their finest and greatest figures.

ROBERT A. MILLIKAN

MAY 11, 1931

VERANUS ALVA MOORE—PIERRE AUGUSTINE FISH

BOTH these men were natives of New York State—Dr. Moore, of the western border in the Lake Ontario basin, and Dr. Fish of the eastern portion in the Hudson River Valley. Both received their early education in the schools and academies of the state, and both were graduates of Cornell University.

Both were called as heads of departments in the original faculty of the New York State Veterinary College at Cornell, and finally both became deans of the college, Dr. Moore succeeding Dr. James Law in 1908, and Dr. Fish succeeding Dr. Moore in 1929. Their friendship, mutual help and confidence in each other continued without a break till the end of life. Both, as if of one spirit, served their country in the World War.

Veranus Alva Moore was born at Houndsfield, Jefferson County, New York, April 13, 1859, and died at Ithaca, New York, on February 11, 1931. His parents were Alva and Antoinette Eastman Moore.

The Moore family, as was common in pioneer days, had mainly the wealth of strong hands and independent character. The father died when the boy was but thirteen years old, thus compelling him to assume the duties of a man very early in life. Young Moore had the manly qualities, and went out to work on a farm to support himself and to help the mother and children. In this farm labor he had the misfortune to step on a nail, which penetrated his foot and set up an infection in the bones which caused him much suffering for the next ten years.

This accident compelled him to come in contact with physicians to obtain relief, and he finally became a patient in Bellevue Hospital, New York. There he saw hospital management and ate the hospital food of the time. He also saw and was operated upon by the famous surgeons of the period, and saw all too often the infections which followed operations. Knowledge of the rôle played by bacteria in infections was known and appreciated by very few, and the pioneer work of Lister in applying to surgery the knowledge of micro-organisms which Pasteur had made known was then not deemed worthy of serious consideration in the minds of most of the medical men.

This contact with physicians and hospitals was destined to have a great influence on Moore's life and to determine later its trend. Meanwhile he made great progress in fundamental education, taught district school, and made a success of it in spite of the crutches he had to use. Never satisfied with present attainments, he passed from the district schools to Mexico Academy, in Oswego County. There he came under the stimulating influence of one of the fine principals (James Gifford) found everywhere as heads in the academies (really junior colleges) of those days. He graduated from the academy in the spring of 1883, and in the fall of that year entered Cornell University, and graduated with his class in 1887.

In the spring of 1887 he had completed the required work for graduation, and was recommended and accepted as assistant with Drs. D. E. Salmon and Theobald Smith in the U. S. Bureau of Animal Industry at Washington.

At this time epoch-making changes were taking place. Meat inspection was being introduced at the great abattoirs, researches were going on to determine the cause and prevention of hog cholera and the Texas fever of cattle. He remained in the bureau for nine years, advancing step by step, until in 1895, when Dr. Theobald Smith went to Harvard, Dr. Moore was made chief of the division of animal pathology. This Washington experience gave him an intimate knowledge of the problems confronting the live-stock interests of the whole country. It also brought him in contact with the leading men of the country both in human and in veterinary medicine.

On the establishment of the New York State Veterinary College in 1896, Dr. Moore was invited to the chair of bacteriology, pathology and meat inspection. Here was carried on the main part of his life work. With his broad experience in the government service, and under the influence of the high ideals of Dr. Law, dean of the college, Dr. Moore entered enthusiastically into the building up of this enterprise. There appeared before him two great goals to attain: To make the preliminary education of students more adequate, and to so teach and train the men in his subjects that they would be on a par with the men in the great profession of human medicine. He knew also the need for and the desire for knowledge of the laws of health, and the ways to avoid such infections as he had suffered from, by the public in general. He therefore gave not only to his students, but in public addresses, in written articles and in his books the information needed by students, the veterinary profession, the stock owners of the country and the general public.

In his thirty-three years of service, he saw the preliminary educational requirements advanced to a full high-school course, and the professional training to four years, as with human medicine. Twenty-one of these thirty-three years of service, he was dean of the college and therefore in a position to bring about more effectively the changes and advances which he saw were needed. He saw the college grow both in students and faculty, and also in financial support. Besides being a great administrator he was a great teacher, and when the alumni went forth into the world, he still had a great interest in them and sent a Christmas letter to each one. In this letter were notes concerning the university as a whole, and of the college in particular, also notes concerning the faculty and the alumni. The alumni were made to feel that the college was to them a real *alma mater,* and welcomed them whenever they returned.

One of the last services which Dean Moore rendered the college was a 47-page pamphlet giving the history of the veterinary work of the university from its beginning, with Dr. James Law as its exponent, until his own retirement in 1929. This history fills one with renewed courage, for it shows how intelligence and devoted service can make advances.

On retiring from the Veterinary College at the age of seventy, Dr. Moore had planned to devote his remaining years to a quiet life of research, and especially to the preparation of a history of veterinary medicine in America. But that was not to be.

From his twelve years on the Ithaca Board of Health, twelve years on the school board, and as trustee of the Ithaca Memorial Hospital and on its medical staff for a long period, he seemed by this training, and his knowledge of the best practice in present-day hospitals, and also the defects of early-day hospitals, and his tried administrative skill, to be just the right person to straighten out the difficulties into which the hospital had become involved with its ever-growing financial deficit and the lack of team work in the staff. Here was a call for help, and to help had been his greatest aim in life. So he put aside the plans for repose and became head of the hospital. Besides his knowledge, he brought to this task that precious faith in human nature which makes all roads straight, and commands everywhere loyalty and devotion to ideals.

All his constructive work and plans were making notable progress when he himself came to need the ministrations of the hospital. There on the morning of February 11, 1931, he passed from the sleep in life to the final sleep in death.

Dr. Moore had a most hospitable home. There were two sons and a daughter, and there came also grandchildren to bring happiness in his advancing years.

In the community, and among the students, the faculty, his scientific and professional friends he was regarded with both respect and affection.

Pierre Augustine Fish was born at Chatham, Columbia County, N. Y., February 17, 1865, and died at his home in Ithaca, New York, on February 19, 1931. His parents were Irvin A. and Margaret Shufelt Fish.

The father was a furniture dealer in the village, and the young man aided in the care of the store when not in school. Besides the village schools he attended the South Berkshire Institute, New Marlboro, Massachusetts, and Hartwick Seminary, near Cooperstown, New York. He entered Cornell University and

graduated with the degree of B.S. in natural history in 1890, and doctor of science in 1894. From 1890 to 1895 he was instructor in vertebrate zoology and neurology in Cornell, and during the summer, from 1891 to 1895, he was instructor in the Woods Hole Marine Biological Laboratory.

Under the inspiring influence of Burt G. Wilder, he became greatly interested in the central nervous system, and fifteen of his earlier papers were upon that subject. His three-dimensional models of small brains, made easily possible by the Born wax-plate reconstruction method, were the first to be made in Cornell. He also applied the Golgi-Cajal silver staining method for elucidating the fine structure, and was very successful with both methods, as may be seen by consulting his paper on the small salamander, Desmognathus, in Vol. X (1895) of the *Journal of Morphology*.

In 1895 Dr. Fish had the opportunity of joining the staff of the Bureau of Animal Industry, and in that year had impressed upon him the tremendous problems involved in the animal husbandry of the country.

On the opening of the Veterinary College at Cornell University in 1896, he, like Dr. Moore, was called as a member of the original faculty. His department was physiology and pharmacology. He threw himself heart and soul into the work. He found the students sadly lacking in preliminary education, and from the beginning worked with Dr. Law and the rest of his colleagues to increase the entrance educational standard, so that the lecture and laboratory work in his subjects might be understood and made so thorough that the alumni would be ready to meet the actual experiences of life in practice with credit to themselves. During the thirty-five years of service to the college he gave class-room instruction to every student who graduated. No one can overestimate the influence of such a teacher upon the minds of these young people whose habits of thinking and study were being formed. He planted in those receptive minds a sense of responsibility, and seriousness, and an appreciation of the sciences on which their life work was based. It was well expressed by one who has since gained distinction: "I look back with gratitude to Dr. Fish for the insight he gave me into the dignity of science, and the way scientific knowledge is gained." Dr. Fish had also great influence for good to the students in his service as secretary of the faculty for twenty-nine years.

He was editor of *The Cornell Veterinarian* for its first five volumes, 1911 to 1916, and of the *Journal* of the American Veterinary-Medical Association from 1915 to 1918. In this editorial field he was especially effective; with a fine literary sense, he put in cogent form the needs and opportunities of the veterinary profession. In his discussions there was fairness and tolerance, but still an insistence on the highest ethical standards.

Dr. Fish was a fellow of the American Association for the Advancement of Science, and of course of the State and National Veterinary Societies.

His numerous writings, including several books, pertain mostly to the physiology of the domestic animals and their relation to human beings. Some of these striking papers deal with veterinary education, and the need of a general fundamental education before entering upon the professional studies.

The keenness of his mind and his comprehension of the requirements for true scientific investigation was impressed on the writer during a joint research extending over three years after he left the U. S. Army service at the end of the world war. Dr. Fish exemplified the ideal researcher. To find the truth was the goal, and no labor was too great to attain that end. He spurned the easy way to cover up ignorance by plausible guesses.

In 1929 when Dr. Moore retired at the age of seventy, he was chosen by the university administration to become dean of the Veterinary College. Dr. Fish was perfectly familiar with the administrative details of the college and shared the ideals of his predecessors; naturally, therefore, it continued its forward march under his wise and considerate leadership.

The students, the faculty and the alumni gave most loyal support, and Dr. Moore in public addresses and in private used his large influence most generously in support of his successor.

A severe cold contracted on a journey to and from Schenectady to give a broadcast on the rise and progress of the veterinary profession in America finally terminated in pneumonia and ended his life, February 19, 1931, eight days after his predecessor and friend, Dr. Moore, had passed away.

Dr. Fish preferred the quiet life of teaching and research to more public activities, but when public duties confronted him he accepted them, and carried them through so finely that every one approved and was satisfied. He was greatly sought for as toastmaster. Every one felt confident that if he presided there would not be a dull moment in the program. His own elegant and witty expressions set the pace and kept the events moving without a hitch.

Dr. Fish's home, with his five children, four daughters and a son, offered a warm welcome to students, colleagues and friends. He was a good neighbor, a good citizen, not only of his own community but of the whole state and nation, and was trusted and honored by all.

The writer would like to express gratitude for the

privilege of teaching these men, for seeing them develop and become beneficent forces in the progress of our country, for having them as colleagues in university work and for their lasting friendship. It was hoped that they might say a kindly word for their old teacher instead of the duty falling upon him to speak for them.

SIMON HENRY GAGE

MEMORIALS

MEMORIAL services for Professor Albert A. Michelson were held on the afternoon of May 18 in Joseph Bond Memorial Chapel of the University of Chicago. Dr. Max Mason, president of the Rockefeller Foundation, spoke on "Professor Michelson as a Scientist." Dr. Mason was for five years president of the University of Chicago, and was previously professor of mathematical physics at the University of Wisconsin. Dr. Henry Gordon Gale, dean of the physical sciences division, who collaborated with Professor Michelson in the study of earth tides, spoke on "Professor Michelson as a man." Dr. Robert Maynard Hutchins, president of the university, spoke on "Professor Michelson's Service to the University of Chicago."

BRONZE busts of four Americans were unveiled on May 14 at the Hall of Fame of New York University: James Monroe, fifth president; Matthew Fontaine Maury, oceanographer; Walt Whitman, poet, and James Abbott McNeill Whistler, artist. The bust of Maury is the work of F. William Sievers, of Richmond, Va., who made the Maury monument in that city. The gift of the United States Daughters of the Confederacy, it was unveiled by Matthew Fontaine Maury, 3rd, great-grandson. Professor S. A. Mitchell, director of the Leander McCormick Observatory of the University of Virginia, and Rear Admiral Walter R. Gherardi, hydrographer of the Bureau of Navigation, spoke in tribute to Maury.

RECENT DEATHS

DR. SAMUEL WILSON PARR, professor emeritus of practical chemistry in the University of Illinois, died on May 16. He was seventy-four years old.

DR. HENRY J. PRENTISS, head of the department of anatomy in the University of Iowa, died on May 17, aged sixty-three years.

Nature announces the following deaths: Dr. J. Anderson, formerly fellow of the London School of Tropical Medicine, later professor of medicine in the University of Hong-kong and recently director of the division of medicine in the Henry Lister Institute at Shanghai, aged fifty-two years; of Professor R. K. Butchart, professor of mathematics in Raffles College, Singapore, and formerly professor of physics in Wilson College, Bombay, on March 30. Senator R. Nasini, professor of chemistry in the University of Pisa, on March 29, aged seventy-five years. Professor Hugh Ryan, professor of chemistry in the University College of Dublin and chief state chemist to the Irish Free State, on March 27, aged fifty-seven years. Mr. T. C. Cantrill, formerly of the Geological Survey of Great Britain, on April 3, aged sixty-three years, and of Sir John de Villiers, noted especially for his work while in charge of the map room at the British Museum and his contributions to geographical and historical literature, on April 2, aged sixty-seven years.

SCIENTIFIC EVENTS

THE LAKESIDE HOSPITAL IN CLEVELAND

THE dedication of the new Lakeside Hospital of the medical center of Western University will take place on June 17. The hospital completes the development of the medical school at the university, which has been made possible by gifts amounting in all to $15,000,000. The buildings include the Schools of Medicine, Nursing, Pharmacy and Dentistry; the Institute of Pathology, Babies and Children, Maternity and Rainbow Hospitals; the Medical Library, the Nurses' Dormitory and the Robb and the Hanna Houses, the latter a private patients' pavilion.

Dr. Hans Zinsser, professor of bacteriology at Harvard University, will make the principal address at the dedication ceremonies. Mr. Samuel Mather, president of Lakeside Hospital for more than thirty years, will preside. Mr. Mather built the School of Medicine and has been the largest single contributor in the development known to the public as the "Cleveland Medical Center." President Robert E. Vinson, of Western Reserve University, will confer honorary degrees.

The dedication will take place in the morning and will be followed by a luncheon under the auspices of the Lakeside Alumni Association in Robb House. In the afternoon are scheduled a scientific session, an inspection of Lakeside Hospital and a visit to the Institute of Pathology. The evening will be occupied with the dinner of the Lakeside Alumni Association.

THE NEW ORLEANS CHAPTER OF THE PAN AMERICAN MEDICAL ASSOCIATION

AT the call of Dr. Arthur Vidrine and Dr. Rigney D'Aunoy, a number of prominent New Orleans physicians met recently in the library of the Charity

Hospital for the purpose of organizing a chapter of the Pan American Medical Association.

The Pan American Medical Association, established ten years ago, has for its objects the promotion of intimate relations, scientific and otherwise, between physicians and surgeons of the western hemisphere, with the idea of advancing medical knowledge and interchanging professors of various medical subjects between different seats of learning, and the development of inter-American scientific literature by means of official publications and an international lending library.

Chapters of the organization exist at present in New York City; Rochester, Minnesota; Baltimore; Atlanta; Miami; Chicago; Detroit; Havana, Cuba; Matanzas, Cuba; Santiago, Cuba; Panama; Merida, Mexico; Mexico City; San José, Costa Rica; Caracas, Venezuela; Sao Paulo, Brazil; Buenos Aires, Argentina, and Bogota, Colombia.

At the organization of the New Orleans chapter the following officers were elected:

President: Dr. Aristides Agramonte.
Vice-President: Dr. P. F. Murphy.
Secretary: Dr. Rigney D'Aunoy.
Treasurer: Dr. Gilbert Anderson.

It was unanimously voted to invite the association to hold its 1932 congress in the City of New Orleans, and Dr. Aristides Agramonte was elected a delegate to the congress to be held in Mexico City from July 12 to 16, 1931, with instructions to extend such an invitation.

THE KENTUCKY ACADEMY OF SCIENCE

THE eighteenth annual meeting of the Kentucky Academy of Science was held on May 2 at Transylvania College, Lexington, Kentucky. The meeting was well attended and much interest shown, thirty-five papers being given in the three divisions. The presidential address, by Dr. V. F. Payne, was entitled "A Chemist Views the Social Sciences." The guest-speaker of the afternoon, Dr. Edmund M. Baehr, of the University of Cincinnati Medical College, spoke on "The Uses of Adversity."

Committees were appointed to consider the matter of junior societies affiliating with the academy; to suggest scientific books to be placed in certain libraries; and to select a suitable place for an academy library.

The following officers were elected:

President: Dr. Anna A. Schnieb, Eastern State Teachers College, Richmond, Ky.
Vice-president: Dr. Charles Hire, Murray State Teachers College, Murray, Ky.
Secretary: Dr. A. M. Peter, Experiment Station, Lexington, Ky.
Treasurer: Prof. W. S. Anderson, Experiment Station, Lexington, Ky.
Council for the American Association for the Advancement of Science: Dr. Austin R. Middleton, University of Louisville, Louisville, Ky.

The academy has now 195 members—the largest membership since its organization. The publication of its *Transactions* has been brought up to date in Volumes 1, 2, 3 and 4.

At the close of the meeting the old Medical Library of Transylvania College was open for inspection. This library contains many rare old books, being the first medical library west of the Allegheny Mountains.

A. M. PETER,
Secretary

THE KANSAS ACADEMY OF SCIENCE

THE sixty-third annual meeting of the Kansas Academy of Science was held at the University of Kansas at Lawrence, on April 24 and 25. General papers and business occupied the forenoons. Over 120 papers were listed, about three fourths of which were given on the afternoon of April 24 in the meetings of the sections of biology, chemistry, physics, psychology and junior academy. The section of entomology met on the afternoon of April 25.

The address by the retiring-president, Dr. Hazel E. Branch, of Wichita University, was given on the evening of April 24, at the banquet. Her subject was "Aims and Opportunities of a Junior Academy of Science in Kansas." The principal address of the session was given later in the evening by Dr. Geo. K. Burgess on the work of the U. S. Bureau of Standards. This was given under the auspices of the University of Kansas Chapter of Sigma Xi.

A Section of Physics and a Junior Academy of Science were organized this year for the first time. Of the few remaining complete sets of the *Transactions* a number were sold to members of the academy in accordance with the policy of the academy not to maintain a library of its own. Last year the exchange rights of the academy were divided between the University of Kansas, the State College and the Fort Hays College, which institutions aid in the publication of the *Transactions*.

The following officers were elected for the ensuing year: *President,* Roger C. Smith, Manhattan; *first vice-president,* Wm. J. Baumgartner, Lawrence; *second vice-president,* J. Willard Hershey, McPherson; *secretary,* George E. Johnson, Manhattan; *treasurer,* Ray Q. Brewster, Lawrence. The new chairmen of the sections are: Biology, Wm. J. Baumgartner, Lawrence; Chemistry, Walter S. Long, Salina; Entomology, Raymond H. Beamer, Lawrence; Physics, E.

V. Floyd, Manhattan; Psychology, Raymond H. Wheeler, Lawrence; Junior Academy, Hazel E. Branch, Wichita. Additional members of the executive council are: Hazel E. Branch; Robert Taft, Lawrence, and J. A. G. Shirk, Pittsburg.

The Kansas Academy at the date of the annual meeting had an active membership of over 300, of which 13 are honorary and 51 are life members.

The meeting in 1932 will be held at McPherson.

GEORGE E. JOHNSON,
Secretary

THE MEDAL MEETING OF THE FRANKLIN INSTITUTE

THE Medal Meeting of the Franklin Institute, Philadelphia, was held on the afternoon of May 20. The Franklin Medallists, Dr. Willis R. Whitney and Sir James Hopwood Jeans, were the speakers. Dr. Whitney's address was entitled "Research: Theory and Practice," and Sir James's "The Origin of the Solar System."

Other medals were presented as follows:

Longstreth Medal to Charles G. Garrard, M.E., Thorn, England.

Wetherill Medals to Thomas Tarvin Gray, president Gray Industrial Laboratories, Newark, New Jersey; Arthur J. Mason, Homewood, Illinois; Henry M. Sutton, Edwin G. Steele and Walter L. Steele, Sutton, Steele and Steele, Inc., Dallas, Texas; Edward C. Wente, Ph.D., Bell Telephone Laboratories, Inc., New York City.

Levy Medal to J. Stuart Foster, Ph.D., assistant professor of physics, McGill University.

Henderson Medal to Arthur Newell Talbot, Sc.D., D. Eng., professor of engineering, emeritus, University of Illinois.

Clark Medal to Daniel J. Young, consulting engineer, Tacoma, Washington.

Potts Medal to Benno Strauss, Ph.D., Essen, Germany.

Cresson Medals to Clinton J. Davisson, Ph.D., and Lester H. Germer, Ph.D., Bell Telephone Laboratories, Inc., New York City; Kotaro Honda, director, Research Institute for Iron, Steel and Other Metals, Tohoku Imperial University, Sendai, Japan, represented by the Honorable K. Midzusawa, first secretary of the Japanese Embassy, Washington, D. C.; Theodore Lyman, Ph.D., director emeritus, Jefferson Physical Laboratory, Harvard University, Cambridge, Massachusetts.

SCIENTIFIC NOTES AND NEWS

RESOLUTIONS were adopted by the Michigan legislature on April 14 citing the accomplishments and contributions to medicine and chemistry of Dr. Frederick G. Novy, professor of bacteriology, University of Michigan Medical School; of Dr. Moses Gomberg, professor of chemistry, University of Michigan, and of Dr. Reuben L. Kahn, of the Michigan State Department of Health. The ceremony was attended by Governor Wilber M. Brucker, Mr. Henry Ford, the supreme court justices, other state officials and many physicians. Governor Brucker, in opening the program, stated that the plan had been originated by Dr. James T. Upjohn, senator from Kalamazoo and chairman of the senate public health committee.

ON the occasion of the presentation on May 22 of the Willard Gibbs Medal to Dr. Phoebus A. Levene, of the Rockefeller Institute for Medical Research, a banquet was held at the Steuben Club, Chicago. Dr. Hermann I. Schlesinger, University of Chicago, chairman of the Chicago section of the American Chemical Society, presided. Dr. Fred G. Koch, University of Chicago, presented the medal, after which Dr. Levene delivered an address on "The Revolt of the Biochemists." Responses were made by Dr. Lawrence V. Redman, president-elect of the society, and by Dr. Robert M. Hutchins, president of the University of Chicago. The Willard Gibbs jurors, forming the committee of award, are: B. S. Hopkins, W. Lee Lewis, S. C. Lind, Julius Stieglitz, W. D. Bancroft, G. Borrowman, Otto Folin, F. C. Whitmore, W. L. Evans, A. A. Noyes, L. V. Redman, Harry Steenbock and H. I. Schlesinger, chairman.

SIR JAMES HOPWOOD JEANS received the honorary degree of doctor of laws from the Johns Hopkins University on May 15. He was presented by Dr. Robert W. Wood, professor of experimental physics, and the degree was conferred by President J. S. Ames. A dinner in honor of Sir James will be given at the Hotel Astor, New York, on Thursday, May 28, under the auspices of the New York Museum of Science and Industry, the American Institute, the American Museum of Natural History, the Amateur Astronomers Association, the New York Academy of Sciences and the *Scientific Monthly*. Professor Michael I. Pupin will preside and will introduce the speaker. Reservations may be obtained from the New York Museum of Science and Industry, 220 East 42nd Street, New York City.

DR. KARL FERDINAND HERZFELD, professor of physics at the Johns Hopkins University, has been designated as the recipient of the Mendel Medal, awarded annually to a Roman Catholic who has achieved distinction in the field of science. The previous winners of the Mendel Medal, which was founded three years ago, are Dr. John A. Kolmer, of the University of Pennsylvania, and Dr. Albert

F. Zahm, chief of the division of aeronautics in the Library of Congress.

THE Oberly Memorial Prize, founded in honor of Eunice Rockwood Oberly, librarian of the Bureau of Plant Industry, who died in 1921, has been awarded to Mr. Everett E. Edwards, economist of the U. S. Department of Agriculture, for his publication entitled "A Bibliography of the History of Agriculture in the United States."

THE British Iron and Steel Institute has awarded the Carnegie Gold Medal for engineering research to Dr. E. Valenta, of the Skoda Works, Pilsen. The work for which the medal was awarded is a book entitled "Heat and Acid-resisting Cast Iron with High Chromium and Carbon Content," and represents the results of two years' experiments. It was issued by the Carnegie Fund as part of the series, Carnegie Memoirs, in connection with the Iron and Steel Institute.

IT is stated in *Nature* that the following have been proposed for election as honorary members of the German Chemical Society: A. Angeli (Florence), E. J. Cohen (Utrecht), W. Ipatiew (Leningrad), Irving Langmuir (Schenectady), H. Le Chatelier (Paris), Sir William Pope (Cambridge), Th. Svedberg (Upsala), and E. Warburg (Berlin).

THE council of the Royal Geographical Society has made the following awards for 1931: The Murchison Grant to Mr. L. M. Nesbitt, for his journey through the Danakil country of Abyssinia; the Back Grant to Col. R. H. Rowe, for his surveys in Nigeria and on the Gold Coast; the Cuthbert Peek Grant to Mr. H. J. L. Beadnell, for his explorations in the Libyan Desert; the Gill Memorial to Mr. Michael Spender, for his studies of the Great Barrier Reef.

THE following elections were made at a meeting of the executive committee of the American section of the Society of Chemical Industry on May 8: *Chairman,* Allen Rogers; *Treasurer,* F. C. R. Hemingway; *Secretary,* Foster D. Snell; *New Members of the Executive Committee,* William Gesell, Robert J. Moore, Arthur Singmaster, Irving Hochstadter and Benjamin T. Brooks.

PROFESSOR HAROLD L. ALLING, of the University of Rochester, was elected president of the New York State Geological Association at the meeting held at Port Henry on May 16. Professor Edward Hoffmeister, also of the University of Rochester, was elected secretary.

OFFICERS of the Physical Society, London, have been elected as follows: *President:* Sir Arthur Eddington; *Secretaries:* Dr. Ezer Griffiths and Dr. Allan Ferguson; *Foreign Secretary:* Professor O. W. Richardson; *Treasurer:* Mr. R. S. Whipple; *Librarian:* Mr. J. H. Brinkworth; *Assistant Secretary:* Dr. J. J. Hedges; *Editor:* Captain C. W. Hume.

DR. FRANK R. LILLIE has been elected dean of the division of biological sciences of the University of Chicago, succeeding Dr. Richard E. Scammon, who is returning to the University of Minnesota to become dean of the medical sciences there.

DR. W. F. HAMILTON has been appointed full professor of physiology at the University of Louisville.

DR. PAUL A. MOODY has been promoted from assistant professor of zoology in the University of Vermont to an associate professorship, and Lyman S. Rowell has been advanced from instructor to the rank of assistant professor of zoology.

Nature reports that at the end of last year, Professor A. A. Ivanoff retired from the position of director of the Observatory in Pulkovo and took over the office of the president-adjoint of the Central Chamber of Weights and Measures, Leningrad.

DR. JAMES H. MEANS, Jackson professor of clinical medicine at the Harvard Medical School, was recently elected a member of the board of regents of the American College of Physicians.

DR. LEWELLYS F. BARKER, of Baltimore, served at the Peter Bent Brigham Hospital, Boston, as annual physician-in-chief, *pro-tempore,* for the week beginning on April 20.

THE J. T. Baker Chemical Company's Analytical Fellowship, Eastern Division, has been awarded to Nelson Allen for the academic year 1931–1932, to work at Princeton University. Mr. Allen received the B.S. degree from Centre College, Kentucky, in 1927, and the M.S. degree from the University of Chicago in 1927, and is now assistant professor at Centre College.

THE Committee on Scientific Research of the American Medical Association has granted $500 to Drs. Harold G. Grayzel, Maxwell Bogin, Hyman Warshall and Mendel Jacobi for continuation of their studies in amyloidosis. The investigations are conducted at the Pediatric Research Laboratory of the Jewish Hospital of Brooklyn.

THE Scientific Club of Winnipeg has awarded its Research Prize of $250, for the most meritorious investigations conducted by a post-graduate student in the University of Manitoba during the last three years, to P. A. Macdonald, Ph.D. The researches of Dr. Macdonald, which were carried out in the department of physics, consisted of studies of the senses of temperature, pain, vision, touch and hearing, with particular reference, in the last three, to the validity of the Weber-Fechner law.

PROFESSOR WILLEM DE SITTER, of the University of Leyden, will visit Mount Wilson Observatory next winter.

SIR FREDERICK WILLIAM ANDREWES, professor of bacteriology at the University of London, recently visited the University of Wisconsin to make observations of work on agglutination problems in progress at the College of Agriculture in the departments of veterinary science and bacteriology.

H. H. WHETZEL, professor of plant pathology at Cornell University, represented the university at the inauguration of the chancellor of the University of Porto Rico on May 20. The new chancellor is Carlos E. Chardon, formerly commissioner of agriculture and labor in the Governor's Cabinet. Professor Whetzel will also give the commencement address at the College of Agriculture at Mayaguez, Porto Rico, on May 25.

DR. NILS A. OLSEN, chief of the Bureau of Agricultural Economics, has been selected by Secretary Hyde to represent the U. S. Department of Agriculture at the conference of wheat-exporting countries, which opened on May 18 at Canada House, London. The Federal Farm Board sent Mr. Sam R. McKelvie as its representative. Dr. Alonzo E. Taylor, of the Food Research Institute of Stanford University, accompanied Mr. McKelvie as technical adviser.

DR. ALBERT W. HERRE, curator of the Zoological Museum of Stanford University, is making preparations for an extensive trip through the Sulu Archipelago, British North Borneo, and parts of the island of Mindanao of the Philippine group, during next year, which will be his regular sabbatical leave. During his trip Dr. Herre expects to gather new fish specimens for the museum at Stanford and to add to scientific knowledge concerning the territory he is to visit.

THERE will be a general assembly of the International Scientific Radio Union in Copenhagen from May 28 to June 8. The following delegates will represent the United States: Messrs. Austin, Briggs, Chaffee, Dellinger, Squier and Wilson. The meeting is being held at the same time and place as the meeting of the official body known as the Technical Consulting Committee on Radio Communication.

SIR JAMES JEANS will deliver the first annual public lecture of the Harvard chapter of Sigma Xi, in Sanders Theater at 8.15 p. m., on May 26, on "The Annihilation of Matter." The lecture is made possible through the courtesy of the Franklin Institute, of Philadelphia, of which Sir James is Franklin medalist for this year.

ON April 17, Dr. R. G. Aitken, director of Lick Observatory, addressed the University of Cincinnati chapter of Sigma Xi at the annual initiation meeting. His subject was "Our Changing Universe."

DR. E. D. MERRILL, director-in-chief of the New York Botanical Garden, delivered an illustrated address on May 21 before the Washington Academy of Sciences on "Plants and Animals of the Philippines and Neighboring Islands—How They Came to be Where They Are."

DR. ROBERT A. MILLIKAN, of the California Institute of Technology, delivered two general university lectures at the University of Oklahoma on May 3 and 4. His subjects were "Science and Religion" and "Opportunities in Science." At a breakfast given in his honor, he addressed members of the physics department and guests on "Recent Developments in the Study of Cosmic Rays."

DR. ARTHUR H. COMPTON, of the University of Chicago, was the chapel speaker at Pennsylvania College for Women, Pittsburgh, on May 8. His subject was "The Human Side of Science."

DR. CLARENCE COOK LITTLE, director of the Roscoe B. Jackson Memorial Laboratory, at Bar Harbor, Maine, formerly president of the Universities of Maine and Michigan, will be the chief speaker at the University of Maine alumni banquet to be held during commencement week on June 6, at which time President Harold S. Boardman will act as toastmaster.

MRS. MAYME I. LOGSDON, associate professor of mathematics at the University of Chicago, was the principal speaker at the annual meeting of the Kentucky Section of the American Mathematical Association held at the University of Kentucky on May 9.

DR. ARNO B. LUCKHARDT, professor of physiology at the University of Chicago, delivered on May 7 and 8 two lectures under the auspices of the Gorgas Medical Society at the School of Medicine of the University of Alabama. The subjects were "The Parathyroid Glands, their Pathology and Physiology" and "High Lights and Shadows in the History of the Discovery of General Anesthesia." After the close of the second lecture Dr. Luckhardt was made an honorary fellow in the Gorgas Medical Society.

ON April 30 and May 1 Dr. Joseph H. Bodine, professor of zoology in the University of Iowa, lectured at the University of Michigan under the auspices of the department of zoology, on the following subjects: "Some Fundamental Problems in the Physiology of Development," "Respiratory

Metabolism of a Developing Egg" and "Cycles or Rhythm in Development." Dr. Fernandus Payne, professor of zoology at the University of Indiana, lectured on May 7 and 8 on "Twentieth Century Advances in Biology," "A General Discussion of Protoplasmic Structure and in particular the Cytoplasmic Structures" and "The Cytoplasmic Structures in Developing Insect Eggs."

AN intersectional meeting of the American Chemical Society is to be held at Pasadena, California, from June 15 to 20, in conjunction with the national meeting of the American Association for the Advancement of Science. This is an unusually important scientific event, which a number of distinguished eastern and foreign speakers will attend. Members of the American Chemical Society are invited to present papers at the sessions held during the latter part of the week. Morning sessions during the earlier days of the meeting will be devoted to symposia.

FOR the first time since 1897 the American Medical Association will meet this year in Philadelphia, the city in which it was founded eighty-four years ago. The new municipal auditorium will be the focal point of the meeting, which will be held from June 8 to 12. With the exception of the meetings of the House of Delegates, all meetings and exhibits will be held there and in the adjoining commercial museum.

THE South Dakota Academy of Science held its sixteenth annual meeting at Eastern State Teachers' College, Madison, South Dakota, on May 8 and 9. Twenty-four papers were read and discussed. The guest speaker was Dr. William A. Riley, chief of the Department of Entomology and Economic Zoology at the University of Minnesota. At the academy dinner on May 8, Dr. Riley spoke on the subject "Warfare of Man with Insects," and on the afternoon of May 9 his subject was "Some Native Parasites." Officers elected for the year 1931–32 are: *President,* B. B. Brackett, professor of electrical engineering, University of South Dakota; *First Vice-president,* George Gilbertson, professor of entomology, South Dakota State College, Brookings; *Second Vice-president,* Ralph E. Dunbar, professor of chemistry, Dakota Wesleyan University, Mitchell; *Secretary-Treasurer,* A. L. Haines, professor of chemistry, University of South Dakota. The next annual meeting will be held at Yankton College, Yankton.

THE first office of the U. S. Public Health Service to be opened in Latin America has been established in Mexico City, at the U. S. consulate, under the direction of Dr. Howard Franklyn Smith, major U. S. M. C.

MR. EDWARD S. HARKNESS will give to Columbia University a library building which it is planned to erect on South Field. It will face the Low Memorial Library, which will be used in the future for rare collections of books and manuscripts, for research workers and for the offices of the president, secretary and trustees of the university. The new library building will be designed to house four million books. Mr. Harkness's gifts to Columbia have exceeded $3,600,000. His gifts last year to charitable and educational institutions amounted to $31,000,000.

A PROGRAM to raise $14,000,000 as a preliminary step to putting the New York University and Bellevue Hospital Medical College development on the East River on a par with the Columbia and Cornell medical centers was tentatively outlined on May 10 by Mr. Percy S. Straus, at the ninety-second anniversary dinner of the Alumni Association of the college. New York University has acquired the land necessary for the project, Mr. Straus said, and the first aim is to raise $5,250,000 for the erection and maintenance of a College of Medicine Building, for which general plans already have been drawn. The medical center plan as a whole calls for ten buildings, some of which are already erected and need only to be enlarged and modernized, while others are in process of building. Mr. Strauss made no estimate of the total cost, but he said that the entire center could be built much more cheaply than were either the Columbia or Cornell centers because Bellevue Hospital has been built and is supported by the city.

THE council of the senate of the University of Cambridge issued a report on the proposed allocation of the "Rockefeller Benefaction." It will be recalled that the International Education Board offered to the university the sum of £700,000, provided the university could secure £479,000, and that the chancellor was able to announce at his installation last year that the university had received promises which would enable them to satisfy this condition. It is proposed to divide the total sum of £1,179,000 as follows: For the university library, £500,000; for agriculture, including the building and equipment of a new laboratory, £162,000; for biochemistry and biophysics, including a professorship of colloidal physics, £121,500; for botany, £108,500; for physiology, £87,700; for zoology, £168,400, and for physics, including the establishment of a professorship of mathematical physics, £30,000.

PRESIDENT HOOVER has written as follows to the Secretary of Agriculture: "In view of the manifest overproduction of wood products, it seems to me it would be of assistance both to the commercial situation and to the real conservation of our forests, if the Department of Agriculture would still further

temporarily restrict the leasing of the national forests for wood production. I believe it would be desirable now to more positively define the limitation of all leases by the Bureau of Forests except for pulp purposes in Alaska, to occasions when a sum of not more than $500 is involved and to cases where some cutting privileges must be given to actually maintain sawmills that are in operation, and this only where the mills can not obtain raw materials elsewhere. The only reason I am moved to make these exceptions is that we should not deprive farmers and small industries of wood supply and should not create local unemployment by inconsiderate action."

THE fifty-first annual meeting of the American Forest Association will be held jointly with the North Carolina Forestry Association at Asheville, North Carolina, on June 3, 4 and 5. Forest conservation as a function of state government will be discussed by Colonel Henry S. Graves, dean of the Yale Forest School, and at one time Chief Forester of the United States. America's land situation, with special reference to the southern Appalachians, will be presented to the conference by Dr. L. C. Gray, in charge of land economics of the United States Department of Agriculture, while R. Y. Stuart, chief forester of the United States, will speak on the national forests in a coordinated program of land use. The subject of state forests and parks will be presented by William G. Howard, superintendent of lands and forests of the State of New York. Dr. Wallace W. Atwood, president of Clark University, will tell of the meaning and place of the national parks. Other speakers will be Dr. Frank R. Oastler, of New York; Dr. Nehemiah Boynton, of Medford, Massachusetts, and James G. McClure, president of the Farmers' Federation.

THE committee on the Costs of Medical Care, of which Secretary Ray Lyman Wilbur is chairman, met in Washington on May 15 and 16. The committee, which has already given four years to the study of the problem, is not a government agency but is underwritten by a group of philanthropic agencies which include the Rockefeller Foundation, the Julius Rosenwald Fund, the Carnegie Foundation, the Russell Sage Foundation, the Twentieth Century Fund, the Milbank Memorial Foundation, the New York Foundation, and the Josiah Macy, Jr., Foundation. Five main questions are being studied by subcommittees: (1) To what extent should an attempt now be made in the United States, with due consideration to economic and geographic variations, to supply all the people's needs for medical service? (2) What general and specialized personnel and what buildings and equipment should be provided for supplying this medical service? (3) How may personnel and equipment be organized so as to insure the highest practicable quality of service and the maximum economies in the use of equipment, and of the time of both practitioner and patient, compatible with adequate return to the persons and agencies providing service? (4) Should there be agencies in local communities to plan for existing and future medical needs and to provide for the coordination of all medical services? (5) To what extent should payment of medical services be based upon individual purchase or upon group purchase?

THE Second International Congress of the History of Science and Technology, as already announced, will take place in London, England, from June 29 to July 3 of this year, with the Science Museum, South Kensington, as its headquarters. The executive committee is hoping to arrange an exhibition of books and periodicals dealing with the history of science and of technology, at the headquarters of the congress throughout the week. Publishing firms interested in this event should send to the assistant honorary secretary of the congress, Science Museum, South Kensington, London, S.W. 7, a list of books which they consider suitable for exhibition. No charge will be made for exhibition-space, but the executive committee will expect publishing firms to undertake the cost of transport of books both to and from the museum.

THE Association of American Medical Colleges announces that the statistical work on the study of the performance of medical students during their first year in medical school is completed. The freshman class of 1929 was recruited from 591 liberal arts colleges. Each one of these colleges may obtain a record of the students they sent into medicine by writing to the secretary of the association, Dr. Fred C. Zapffe, 25 East Washington Street, Chicago, Illinois. The association undertook this work to establish cooperation between the liberal arts and the medical colleges with the expectation that a better understanding of aims of both groups will result.

THE New York State College of Forestry, Syracuse, New York, has been notified by Sven Petrini, the secretary-general of the International Union of Forest Research organizations, Experimentalfaltet, Sweden, of its election to membership in the International Union of Forest Research organizations. In addition to the New York State College of Forestry there are five other forestry institutions belonging to the union in the United States—the Harvard Forest; the Yale School of Forestry; School of Forestry and Conservation, University of Michigan; Forest Soil Laboratory, Cornell University, and the California

Forest School of the University of California. Germany has the largest membership embracing eight institutions, Great Britain has six and other foreign countries from one to three memberships.

THE Department of Geology at the University of Illinois, through the courtesy of Dr. John B. Reeside, Jr., of the United States Geological Survey, has received a gift from the United States National Museum of a collection of Mesozoic plant and invertebrate fossils.

WE learn from *Nature* that the Pontifical Academy of Sciences (Nuovi Lincei) is offering a prize of 10,000 lire for a critical dissertation on the law of Mendel and the chromosome theory. Essays must be unpublished and may be written in English, French, German, Spanish, Italian, or Latin. They may be signed or written under a pseudonym, and must reach the Pontifical Academy of Sciences, The Vatican City, before October 30. The award will be announced at the first meeting of the academy in December.

DISCUSSION

BIOLOGICAL ABSTRACTS

BIOLOGICAL ABSTRACTS is intended to serve as a comprehensive abstracting journal of the entire literature of biological research of the world. The first number appeared in December, 1926, and the fifth volume is now in course of publication. Its subscription price is $15.00 per volume. It is the only publication in the world that pretends to cover the entire field of biology. Our German colleagues in order to obtain an equally comprehensive service would have to subscribe to ten abstract journals in theoretical biology at a cost of $382.50 per year, and to some at least of the journals in applied biology which collectively cost $305.00 a year additional. There would be, of course, an immense amount of duplication in such a collection of the admirable German abstracting journals. But the comparison will serve at least to indicate what an enormous undertaking it has been to organize a single journal which eliminates all duplication. If the performance of *Biological Abstracts* to date gives good promise of realization of its aim, there would be few to deny that it is a contribution to the service of biological research worthy of a high institutional rank.

It is not necessary to argue the case of the need of a reference system that will be something more than merely bibliographic before biologists of any one of the numerous persuasions. Whether those who grew up under the relatively simple conditions existing before the war are more convinced than those to whom the present intense worldwide activity and cooperation in scientific research seems of the order of nature is perhaps a matter of indifference, for neither the veterans nor the recruits are able without the aid of a comprehensive abstracting journal to view the entire biological front with its thousands of media of publication, whatever may be possible within a small field of investigation. And it is becoming increasingly important in genetics, in physiology, biochemistry and biophysics, in pathology and bacteriology, in ecology, in cytology and other disciplines within biology, to know not only what is being done by the zoologists, but also by the botanists, and not only in academic institutions and museums, but also in the medical schools, the institutes of agriculture, in the fisheries and oceanographic institutes, to name only some of the organizations in applied biology. Each piece of work has some taxonomic implication, so that the bad tradition that has separated systematists and workers in fields of more general biology should be broken down. If *Biological Abstracts* is really comprehensive, it is not only a great convenience, but it is a great unifying force in the life-sciences, and hence an agency of scientific progress.

It has been said that it is an impossible undertaking, but at present about 5,500 periodical publications out of a total of about 6,000 are being abstracted for their biological content. It will not require much extension to make the survey practically complete. Abstracts are furnished in part by authors, and to a great extent by some 3,000 collaborators scattered in all countries where biological research is in progress. Yet the resulting volumes, owing to special format and thin paper, are not unwieldy. The progress that has been made in these five years in completeness of references is guarantee of a fully satisfactory representation of the literature. The main criticism that can be made up to the present is the slowness in appearance of the index numbers, without which use of the volumes is rather painfully slow. But the recent appearance of the index number to the first volume is evidence that the difficulties connected with this essential part of the service have been overcome. It is to be hoped that the index numbers of the succeeding volumes will now be expedited. Already *Biological Abstracts* has a subscription list of over 3,000, which is greater than that of any other biological publication in America. It has been demonstrated not only that

the undertaking is possible but that it is very widely welcomed.

Biological science does not have back of it extensive, well-organized and highly paying commercial organizations dependent on the progress of the science and contributing to its development and support, such as exist in the case of chemistry, for instance. Although the applied aspects of biology in medicine, in agriculture and fisheries are of at least equal human significance they are not organized as commercial enterprises, and hence can not contribute directly to the support of *Biological Abstracts*, as can the commercially organized chemical industries to *Chemical Abstracts*. For a long time to come, then, *Biological Abstracts*, if it is to continue, must depend on the support of enlightened philanthropy. At present its earned income from subscriptions and other sources is sufficient to pay manufacturing costs only. This agrees with the original estimates drawn up before the project was under way. But the great items of cost, consisting of editorial, indexing, bibliographic, secretarial and clerical services, which make up from two thirds to three quarters of any adequate operating budget, must be specially provided. This constitutes an enormous "overhead" which exists whether the subscription list be large or small. It is estimated to amount to over $100,000 for 1931. This is obviously a situation in which every biologist can help, by his subscription. It is, however, not expected that even with the largest list of subscriptions practically possible, the overhead charges can be paid by the receipts of the journal.

The whole enterprise of biological research is, however, so vast and its human usefulness so inestimably great that such a sum seems to be only a small tax upon it; indeed, almost vanishingly small compared with the immense sums required for primary costs of the research and original publication. If the journal should acquire still more of an international character, the tax on American biologists and American philanthropy might be correspondingly reduced; but it seems to the writer that American biologists and philanthropists who have the advancement of science at heart should not withdraw their support until the future of this comprehensive abstracting service is adequately safeguarded.

FRANK R. LILLIE

UNIVERSITY OF CHICAGO

TWISTED TREES

I HAVE read with interest the notes in SCIENCE for February 13 and March 27 dealing with trees with twisted bark. My observations covering a large part of the province of Ontario, Canada, may be of interest in this connection. In this region I have often noted the twist of evergreens, especially of the cedar (*Thuja occidentalis*), white pine (*Pinus strobus*), Norway pine (*Pinus resinosa*) and Jack pine (*Pinus divaricata*). I have never noted it on a "hardwood" in the region. In the cedar the twist is very common, straight-grained trees being far less abundant than twisted-grained. Last summer I camped on an island in Lake Kahnipiminanikok, and my party amused itself one rainy day noting this twist on cedars, some one having discovered the predominance of right-handed twists. We counted (from my notes) 312 cedars on the island; of these 219 were twisted; of these 187 were right-handed twists. Later an Indian emphasized the need of straight-grained cedars in the hewing of paddles, and the difficulty of obtaining such grains in that vicinity. He also pointed out the fact that the twist is more common in large trees than in young ones, indicating that this character is acquired by some environmental factor. This twist is not alone in the bark, but in the wood as well. It is frequently so extreme as to be a spiral. Among the white and Norway pines the twist is far more common in trees exposed to severe weather conditions, especially to strong winds. Thus I noted that twisted trees occurred more commonly on exposed rocky cliffs and small, open islands, where they receive the full brunt of winter gales. A twisted tree in the heart of the forest is quite rare. But why do they twist so predominantly to the right?

A. R. CAHN

URBANA, ILLINOIS

THE MECHANISM OF CROSSING-OVER

DR. SAX[1] has recently put forward a theory that crossing-over is due to the breaking of chiasmata in the course of terminalization. This theory is based on a suggestion of mine[2] that "crossing-over is occasioned by breaking of chiasmata." Moreover, Sax uses diagrams and terminology that are borrowed from my studies and therefore imply an interpretation in accordance with my findings.

I should like to point out therefore that I do not consider the original conjecture in any way supported by Sax's observations. I discarded the idea a year ago for reasons that are described in the accounts of studies conducted in this laboratory by Erlanson,[3] Philp and Huskins[4] and myself.[5,6,7,8,9] Briefly, the

[1] K. Sax, "Chromosome Structure and the Mechanism of Crossing-over," *J. Arnold Arboretum*, 11: 193–220, 1930.

[2] C. D. Darlington, "Meiosis in Polyploids, II. Aneuploid Hyacinths," *J. Genet.*, 21: 17–56 (see p. 52), 1929.

[3] E. W. Erlanson, "Chromosome Organisation in *Rosa*." *Cytologia*, 2 (in the press).

[4] J. Philp and C. L. Huskins, "The Cytology of *Matthiola incana* R. Br.," (especially in relation to the inheritance of double flowers), *J. Genet.*, 24 (in the press).

reasons are twofold: (1) Frequencies of chromosome pairing and of kinds of configurations occur that can be predicted only on the assumption that chiasmata do *not* break in terminalization: (2) configurations occur in polyploids and structural hybrids that are only compatible with the alternative hypothesis that crossing-over has preceded (and determined) chiasma formation. Sax's genetical remarks might be taken to favor either hypothesis—for there is no decisive evidence between them. They are however vitiated as evidence by his using the word "chromosome" in three different senses, *e.g.*, p. 209, l. 25 to mean one chromatid, l. 27 (?) two chromatids, and l. 28, four chromatids.

C. D. DARLINGTON
JOHN INNES HORTICULTURAL INSTITUTION,
LONDON

ADVANCES IN THE BIOLOGICAL SCIENCES

THE inclusion of the notes compiled by Science Service on the advances made in various branches of science during each year is a valuable feature of your paper. Permit me, however, please, to make a few comments on two of the 1930 items.

The tannic acid treatment of burns, originated by Dr. E. C. Davidson, of Detroit, was first published by him as far back as 1925. In 1929 the British Medical Research Council published a memoir by Mr. W. C. Wilson, of Edinburgh, and the results given in it "wholly confirm the claims made by Dr. Davidson for the tannic acid treatment."

Another item given in the 1930 list is the use of a neon lamp, connected in parallel with a condenser, to measure a small electric current. This device was applied by Dr. H. C. Rentschler, director of the research laboratories of the Westinghouse Lamp Company, to the measurement of the photo-electric current from a uranium cell, sensitive to ultra-violet light only. As far as I know his work was published during 1930. The same method, with minor differences, had, however, been published by Dr. J. H. J. Poole, of Trinity College, Dublin, in 1928, in the *Scientific Proceedings* of the Royal Dublin Society. When reading his paper before the society, Dr. Poole established that the rate of flashing was proportional to the photo-electric current from a sodium cell, and demonstrated the flashing to the audience by means of a two-valve amplifier, so that each flash was heard as a rap.

Through the courtesy of Dr. Poole I was able similarly to demonstrate the flashing in a course of lectures in London University in May, 1929.

At the joint discussion on photo-electric cells, held by the Physical and Optical Societies in London on June 4, 1930, J. H. J. Poole and H. H. Poole communicated a further paper on the neon discharge tube method, and gave the results obtained for the absorption coefficient of the water of Lough Bray, in the Dublin Mountains, during August, 1929. In the autumn of the same year Dr. H. H. Poole and the writer collaborated in the use of the method for submarine photometry. For this it has many advantages, as it integrates the current over a suitable time interval and permits one to obtain correct values in which the effect due to dancing of the waves is averaged out. About the same time, and since, we used the apparatus for measuring in various situations the color of daylight (and other light sources, carbon arc, etc.) with a photo-electric cell, and more recently for the study of a mercury vapor arc. Accounts of these applications are now in the press.

The method has many uses. It appears to have been invented by Dr. J. H. J. Poole, and later by Dr. Rentschler, quite independently.

W. R. G. ATKINS
MARINE BIOLOGICAL LABORATORY,
PLYMOUTH, ENGLAND

LITERATURE RELATING TO COD LIVER OIL

IN our recent "Report to the Empire Marketing Board on the Relative Values of Cod Liver Oils from Various Sources" (E. M. B. 35) an unfortunate blunder has arisen during the revision and proof-reading of the introductory chapter.

Reference 6 on page 9 is incorrectly given and should, of course, refer to the important paper by Professor Steenbock and his colleague Dr. Boutwell in the *Journal of Biological Chemistry* (1920, vol. 42, p. 131).

Immediately our attention was drawn to the error we wrote to Professor Steenbock expressing our great regret and, although he desired that we should give no further attention to the matter, we feel that we must override his wishes and make public our correction of the mistake.

On the same page the reference to Professor E. Mellanby's pioneer researches on the causation of rickets might suggest that his results were not published before 1921. We were, of course, well aware of his earlier contributions to the subject but thought it better, in a brief summary, to refer the reader to the full account of his earlier work which was published by the Medical Research Council.

[5] C. D. Darlington, "A Cytological Demonstration of 'Genetic' Crossing-over," *Proc.* Roy. Soc., 107: 50–59, 1930.

[6] C. D. Darlington, "Studies in *Fritillaria* III: Chiasma Frequency and Chromosome Pairing in *Fritillaria imperialis*," *Cytologia*, 2: 37–55, 1930.

[7] C. D. Darlington, "Meiosis in Diploid and Tetraploid *Primula sinensis*," *J. Genet.*, 24: 65–96, 1931.

[8] C. D. Darlington, "Meiosis," *Biol. Rev.* 6, (in the press).

[9] C. D. Darlington, "The Cytological Theory of Inheritance in *Oenothera*," *J. Genet.*, (in the press).

We would like to express our apologies to these distinguished investigators.

J. C. DRUMMOND
UNIVERSITY COLLEGE, LONDON
T. P. HILDITCH
UNIVERSITY OF LIVERPOOL

INSCRIPTIONS FOR A SCIENTIFIC BUILDING

URSINUS COLLEGE is erecting a new building for instruction and research in the sciences. It will be a rather imposing structure for a small college. On the face of the building at either side above the front entrance inscriptions are to be cut. The space permits each text to consist of about seventy letters. They might be quotations or original compositions.

I am seeking suitable inscriptions and shall be under obligations to any reader of SCIENCE who may be willing to send suggestions.

GEORGE L. OMWAKE,
President
URSINUS COLLEGE,
COLLEGEVILLE, PENNSYLVANIA

SCIENTIFIC BOOKS

A History of Entomology. By E. O. ESSIG. The Macmillan Company, New York, 1931. 1,029 pp., $10.00.

DESPITE its inclusive title, this work treats primarily of the history and development of applied entomology in California. It is replete with valuable and interesting information on this and related subjects.

Should one insist on the definition of history as "a narrative of events," then some of the contents of the work could not qualify in that category. Nevertheless, the evidence of intelligence in the choice of material, the conscientious accuracy and completeness of the data included, and the evident effort of the author to render the work of the utmost use to the reader, all evoke his admiration and respect.

In perusing these pages, one is certain to be deeply impressed, too, with the early and leading part taken by the people of California in the advancement of applied entomology and its concomitant activities in America. Settled as it was, after the hegira of 1849, by a virile race of hardy pioneers, California's specialized forms of agriculture, which almost from the first have produced crops of high intrinsic value, together with the great wealth produced by the swift development of her mineral resources, soon gave her a surplus of vigor and power which account in large part for the courageous manner in which she has attacked not only her entomological problems but her civic and social emergencies as well. Thus she was the first of all the states (Howard[1]) to protect herself (in 1880) by legislation and quarantine against the introduction of new insect pests. And this is but one illustration of her forward-looking and aggressive spirit. Her leadership among the states, in the investigation of fumigants and chemicals for the destruction of insect foes, and her early successful adoption of biological methods of insect control, all evince the same qualities of vitality and courage as were exhibited so marvelously after the great earthquake and fire of 1906.

If, as seems possible, it be true that several serious insect pests such as the cottony-cushion scale, San José scale, black scale, etc., gained entrance to America through the activities of the early horticulturists of California, the world will learn through these recent expositions of entomological history that she has atoned nobly for such unintentional lapses by her innumerable valuable contributions to the control of these and of other insect pests for which she has been in no way responsible.

The opening chapter of the present work treats very briefly of the paleoentomology of California, while the second chapter is devoted to an entomology of the Californian Indians. This occupies some thirty-six pages and is of general interest but has a marked ethnologic flavor. After a few pages giving the general historical background, the origin and present status of the Californian institutions pertaining to entomology are related in which discussion the universities and colleges are given individual attention. Then follows a section comprising 192 pages, discussing the history of "The More Important Orchard Mites and Insects of California." With one exception (that pertaining to biography), this chapter is the longest in the book and is considerably more comprehensive than is indicated by its title. For instance, it includes important pests of forage crops, ornamentals, vegetables and even insects affecting health such as the fleas and some mosquitoes. A circumstantial account of the history of sericulture in California and a brief discussion of apiculture in the state conclude the chapter.

The matter is arranged ordinally, beginning with the Acarina, without regard to chronological sequence. A brief history of each species is given together with its common and scientific names. This often includes the formulae used in control, and numerous refer-

[1] L. O. Howard, "A History of Applied Entomology," Smithsonian Inst., Nov. 29, 1930.

ences to the literature of the insect are carried in voluminous footnotes.

The fascinating subject of the biological control of insect pests is treated extensively in a chapter of 129 pages. Here, of course, is found the somewhat hackneyed but ever dramatic tale of the introduction of the Australian ladybird by Koebele and Coquillett in 1888, and the consequent salvation of the citrus fruit culture in California. It is not too much to say that the spectacular success of this project has had a far-reaching and profound effect on the development of economic entomology in America. The idea of "a bug to eat a bug" is still so popular that the administrative officials of the Federal Bureau of Entomology are sometimes compelled to apply the brakes to ill-advised popular movements which would appropriate government funds in excess of those which can be legitimately expended for the introduction of useful parasitic insect enemies.

In passing it might be of interest to remark that the arrangement of the present work in chapters which bear no chronological relation to each other has compelled the author to numerous repetitions which would have been avoided had it been possible to carry his narrative through consistently as a continuous story. For instance, the story of the Australian ladybird introduction is repeated, at least in part, in five different chapters of the book, thus entailing both additional labor and expense for the author and a lessening of sustained interest on the part of the reader. However, it is but just to the author to say that the difficulties to be surmounted in preparing such a chronological narrative are many and formidable and had this plan been adopted it might have resulted in a less complete exposition of the subject.

Extensive tabulations showing the various introductions of the Australian ladybird and other coccinellids into California, as well as much similar information, are given in this chapter. The Hymenopterous parasites are allotted more than 50 pages in which those of the scale insects and mealybugs are most fully treated, including members of the Aphelinidae and Encyrtidae. There is included at this point an interesting account of the collectors of foreign parasites, including the work of Albert Koebele, George Compere, Henry Viereck, E. J. Vosler and others. It ends with a long tabulation of "The Most Important Parasitic and Predacious Insects Introduced into California Following the Work of A. Koebele," and a discussion of California insectaries. Following this there are one hundred pages devoted to a history of the commoner insecticides and their application principally from a Californian point of view. In these, formulae both ancient and modern are discussed. Quite properly, the discovery and development in 1886 of the fumigation of citrus trees with hydrocyanic acid gas for the destruction of scale insects, by Daniel W. Coquillett, an agent of the Federal Department of Agriculture, is fully related. Since the invaluable services of this able man both to applied entomology and its taxonomy have not hitherto been given appropriate recognition it is gratifying to see them assigned a prominent place in this historical work. This chapter ends with an account of the development of fumigation under vacuum, by Sasscer and Mackie.

As California pioneered in entomological legislation it would have been disappointing had a succinct history of this phase of entomology been omitted in the present work, but we find some 38 pages devoted to this subject. Here is told the origin of the organizations which preceded the creation of the State Department of Agriculture in 1919 and the functions they performed in protecting the rapidly developing agriculture of the state. Suitable mention is made of the integration of state and federal forces.

In none of his many excellent writings has Dr. Essig performed a more acceptable and valuable service to science at large than in the compilation of the 270 pages of biographical matter which comprise Chapter IX of the present work. Well-prepared biographies are always valuable, but until now the biographical data relating to the entomologists, a group of more or less obscure men at best, have remained scattered through the general literature of the subject or hidden away in inaccessible records. The collation and publication of this information will be welcomed by all and the more so because of the good portraits which accompany it. Regarding these biographies the author says, "It was my original purpose to include in this chapter a short biographical sketch of every one who had contributed to the development of entomology in California and it is a real disappointment to me not to be able to carry out my plans." When it is known, however, that the chapter includes most of the notables in entomological history who have had the most remote connection with California, from Linnaeus and Fabricius in the middle of the eighteenth century to L. O. Howard and Herbert Osborn, of the present day, it would seem that Dr. Essig might rest content that something remains for future historians to do. Very many of the biographies included are accompanied by more or less full bibliographies of the individual concerned.

The book terminates with "A Chronological Table Showing the Development and Progress of Entomology in Relation to History and Other Sciences." The arrangement of this material is somewhat puzzling as in both the natal column on the left, and the mortuary data headed "Deaths" on the right, two separate dates are recorded for each individual; it

would appear from this that such distinguished personages as Columbus, da Vinci and Copernicus were born twice! But, what is far worse, that such benefactors as Peter the Great, Hans Sloan and Galileo Galilei suffered a double death! It used to be said that "oil and water will not mix," but we entomologists now know very well that there is such a thing as a miscible oil. However, history and fiction are alleged to be utterly incompatible—I wonder? Seriously speaking, this chronological table will be of great service to students of entomological history.

This is a well-built book. It is bound in keratol, printed on thin, calendered paper, and although it is but little more than one and one half inches thick, it weighs about three pounds!

W. R. WALTON

BUREAU OF ENTOMOLOGY,
WASHINGTON, D. C.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE SHAPOMETER: A DEVICE FOR MEASURING THE SHAPES OF PEBBLES

FOR several years the senior author has been interested in methods used for calibrating the shapes of pebbles, and he has been working on a method which could be used by a number of workers and yield comparable results. In a paper published recently[1] he described a method which has been tested during the last two years by a number of students in connection with their research problems. The validity of the method became apparent not only for grains of sand size but also for pebbles, cobbles and small boulders. A method by which the shapes of sand grains may be measured by using a petrographic or binocular microscope was suggested in the paper just cited, and the statement was made that the instrument to be used in measuring pebbles and cobbles would be described later. The instrument is called a *"shapometer."* The general idea of the shapometer was suggested to the junior author who perfected it and measured several hundred pebbles to prove the utility of the instrument.

According to the method proposed by Tester,[2] the shape of any pebble measured is the ratio of abrasion to the original angularity, and measurements are made of the remnant sides or edges and of the projected original edges. Hence, a simple instrument with three or four straight edges calibrated in small units and with movable slides or pivots will suffice for measurement. Fig. 1 is a generalized sketch of the shapometer with 1 mm divisions on the main parts. The lower member "A" is graduated and slotted as shown, to permit an easy sliding of scale 'C.' Scale 'B' is attached to 'A' by a fixed

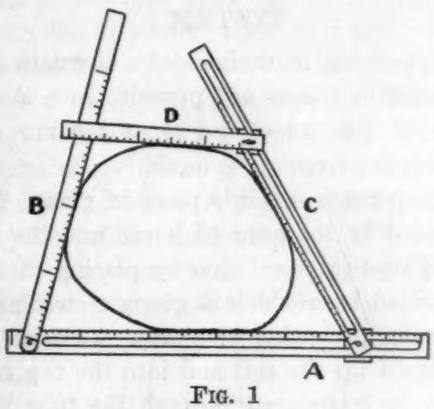


FIG. 1

pivot and although it will not slide it will make with "A" any angle desired. Scales 'B,' and 'D' are graduated in the same units as 'A.' Experience shows a millimeter scale to be satisfactory. Scale 'B' is solid, but 'C' is slotted to permit the attachment of a fourth scale 'D.' Scale 'D' is provided with a screw so it can be removed, but it is very useful when measuring a section of a pebble which has four or more principal surfaces.

The instrument may be constructed of stiff cardboard, celluloid or light weight metal. The writers have used the common 6 inch celluloid millimeter scales with considerable success, but aluminum bars are ideal. The scale 'A' is 7 inches long over-all length.

[1] A. C. Tester. "The Measurement of the Shapes of Rock Particles," *Jour. Sed. Petrol.*, Vol. 1, No. 1, 1931.
[2] *Op. cit.*

TABLE I

Surface or side measured	A		B		C		D	
Projected lengths or original edge	44		28		31		22	
Length of remnant edge	14		9		10		8	
Ratio of remnant to original edge (angularity ratio)	$(\frac{14}{44})$	31.8%	$(\frac{9}{28})$	32.1%	$(\frac{10}{31})$	32.2%	$(\frac{8}{22})$	36.3%
Ratio of abraded to original edge (abrasion ratio)	$(\frac{30}{44})$	68.2%	$(\frac{19}{28})$	67.9%	$(\frac{21}{31})$	67.8%	$(\frac{14}{22})$	63.7%

Average abrasion ratio or roundness = 66.9%.

The use of the shapometer is illustrated best by a study of Fig. 1 in which a pebble is shown in one of the three principal positions. The graduations on the scale are in millimeters. The following table shows the nature of the readings.

The junior writer has found that a smaller shapometer with a range of 75 mm instead of 150 mm is advantageous for measuring particles below 40 mm. The greater ease of handling the smaller instrument insures more accurate and rapid results.

ALLEN C. TESTER
H. X BAY

SEDIMENTATION LABORATORY,
STATE UNIVERSITY OF IOWA

APPARATUS TO CIRCULATE LIQUID UNDER CONSTANT PRESSURE IN A CLOSED SYSTEM

THIS apparatus is designed to circulate a liquid and to maintain a constant pressure in a sterile system, without the use of joints or moving parts in contact with the circulating liquid.

The apparatus is a single piece of glass. Pressure is maintained by the head of liquid and the liquid is raised and kept in circulation by placing the apparatus on a tilted base which is given a circular motion without being permitted to rotate. This motion carries the liquid up the coil and into the top reservoir. Gases can be introduced through the tube half way up the coil, and an internal pressure can be maintained, if desired, by the displacement of water or other fluid by the exhaust gases.

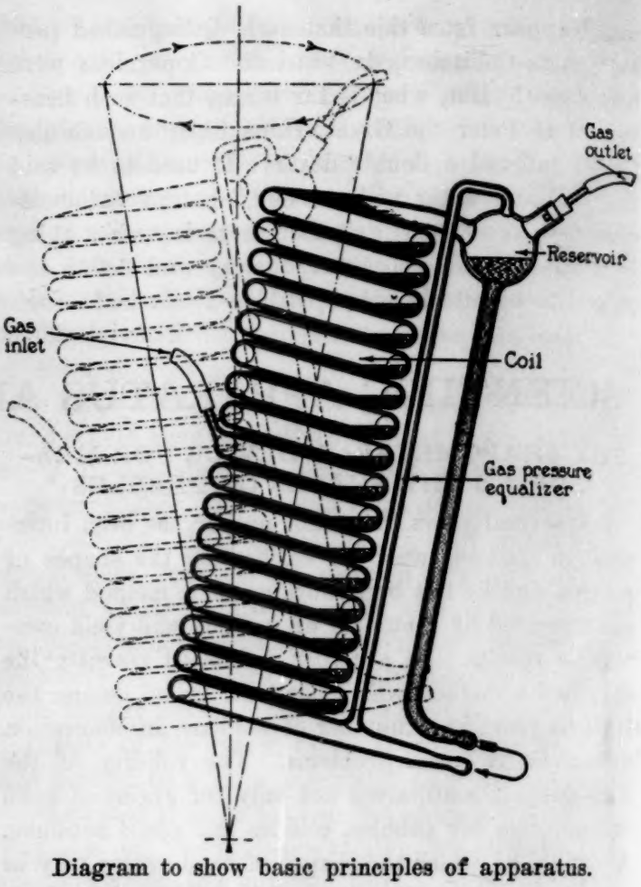


Diagram to show basic principles of apparatus.

DIVISION OF EXPERIMENTAL SURGERY,
ROCKEFELLER INSTITUTE
FOR MEDICAL RESEARCH

SPECIAL ARTICLES

GROWTH OF PLANTS UNDER CONTINUOUS LIGHT

STUDIES carried on by the writer since 1926 with plants illuminated both day and night seem of sufficient interest to report upon briefly at this time. Others who have tried somewhat similar although not exactly the same experiments[1] seem not to have secured just the results which have been so apparent in my work. Preliminary accounts of my studies were made at meetings of the Southwestern Division of the American Association for the Advancement of Science in Santa Fe, 1927, and in Flagstaff, 1928. The experiments are being continued, and a full account will be published at a later time.

Plants, chiefly annuals, have been grown in the greenhouse under natural light in the daytime and, in addition, during both day and night they have had the light of two 100-watt Mazda lamps suspended above the bench at a distance of four feet—the lamps provided with an overhead reflector. Controls, shielded from the artificial light, are growing in the same room of the greenhouse on the same bench at a distance of about ten feet. A total of nearly one hundred species have been worked with, some of them during two or more seasons if first results seemed doubtful. The list includes common garden vegetables, grains, weeds, native herbs and garden ornamentals.

In general, the experimental plants are taller than the controls at all times during the entire growth period, this increased height being due to elongation of internodes. Frequently the experimental plants are slender-stemmed and have a decumbent habit. Flowering is usually hastened under continuous light but in a few species is completely inhibited. Plants of some species reach full adult stature, come to blossom, and produce fruit and seed while the check plants are still in the rosette stage close to the ground.

The root system in plants of the experimental series is invariably less extensive than that of the controls; roots are smaller, shorter, and have fewer branches. Thickened taproots do not develop.

[1] J. Adams, *Amer. Jour. Bot.*, 12: 398, 1925. R. B. Harvey, *Bot. Gaz.*, 74: 447, 1922.

Leaves of plants under continuous illumination often show no modification but in a considerable proportion of the species studied they have smaller, thinner blades, and often longer petioles. Leaves of monocotyledons tend to be very much lengthened as do the sinuately deeply-cleft leaves of certain Hydrophyllaceae. Reduction in leaf size is especially noticeable in certain members of the chickweed family, and this with the thin stems and greatly lengthened internodes and frequent paleness of color gives a suggestion of etiolation. But only a few species are sufficiently pale or show the leaves so much reduced as to make the similarity to etiolated plants very pronounced. The greatly lengthened internodes are, however, a practically constant feature.

Internally, the stems of plants grown under continuous light show a thinner cortex, less vascular tissue (especially phloem), and a relatively larger pith than the controls. Leaf-blades in cross-section look as if derived from plants grown in the shade, usually having a single layer of palisade and with more and larger intercellular spaces than the plants grown under ordinary greenhouse conditions. Leaf cells are smaller, hence the leaves are thinner. Roots of the plants of the experimental series show slight development of phloem but otherwise are of usual structure, except that as previously noted they are small and short and with few branches.

FRANCIS RAMALEY

UNIVERSITY OF COLORADO

A MAXIMUM POINT IN AN EFFECT OF PROLONGED X-RAY IRRADIATION UPON DROSOPHILA LARVAE

IN a previous report[1] from this laboratory certain effects of X-ray irradiation upon drosophila larvae have been presented. It was found that under the given conditions the mean duration of the prepupal period, φ, (the interval, expressed in days, between the laying of the egg and the formation of the pupa) was an increasing function of the period of irradiation, t. In the work referred to the maximum irradiation interval was six hours, which was taken as a limit to the time for subjection of the larvae to the unnatural environment as well as to the continuous operation of the X-ray tube. As has been stated, an increase of X-ray radiation power was not possible at that time. In view of these circumstances observations were made concerning the effect of radiations of longer wave length with the idea of obtaining more extensive changes. We do not assume, however, that effects of radiations of different wave length are the same. Accordingly, radiations filtered with only 3 mm of cardboard were employed, using a Coolidge air-cooled tube (tungsten target) impressed with a potential difference corresponding to a spark-gap reading (between 12.5 cm spheres) of 2.0 cm and a tube current of 8.0 M.A. with otherwise the same X-ray apparatus.

Larvae were prepared for irradiation and maintained in the same manner as in the work previously reported,[1] except that they were irradiated in wells in paraffin blocks 125 mm square by 35 mm in thickness. The wells were cylindrical (25 mm in diameter and 5 mm deep) and were situated with axis 25 mm from a corner on a diagonal of a square face of the block—one such well in each block. Just prior to irradiations approximately 200 larvae were placed in each of eight such wells and covered with perforated filter-paper permeated with paraffin as described in the earlier work.[1] Four of these blocks were then placed with wells uppermost upon the same plane with the adjacent corner at a point 30 cm vertically beneath the center of the target of the X-ray tube. Periods of irradiation were so chosen that the following scheme could be employed.

The irradiation intervals were seven successive multiples of 35 minutes up to 245 minutes; so that, with the exception of the last, they could be arranged in pairs whose sum was 245 minutes. Accordingly, with just four blocks in position at all times (as described above) it was possible by means of substitution at

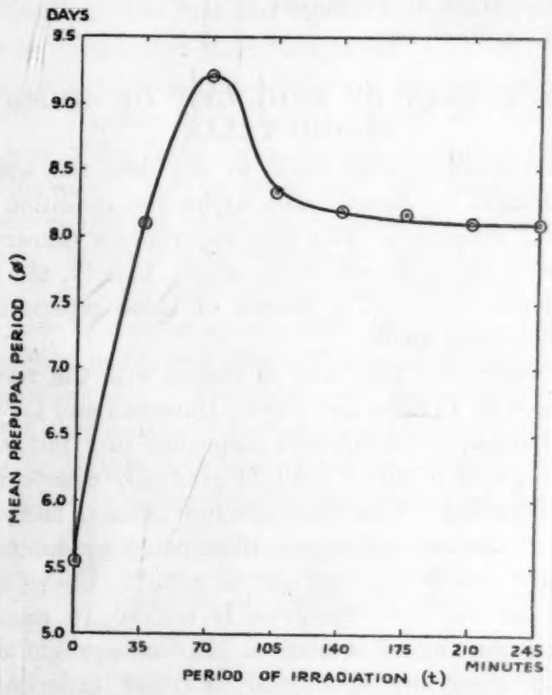


FIG. 1

the proper time of the other member of each pair to complete all the irradiations within the same interval employed for the longest. The eighth block was kept in the same room but shielded from the radiations, and thus corresponded to a zero irradiation interval or control.

[1] R. Hussey, W. R. Thompson and E. T. Calhoun, SCIENCE, 66: 65–66, 1927.

Eight independent experiments of this sort were made, the orientation of the respective blocks being successively interchanged as was also the order of irradiation of the respective pairs so that positional or such chronological difference might not influence the means of all these results. These mean values of φ together with the corresponding periods of irradiation, t, are given in the table and represented

TABLE I

t (in minutes)	φ (in days)	A.D.	t (in minutes)	φ (in days)	A.D.
0	5.55	.06	140	8.20	.07
35	8.09	.11	175	8.18	.07
70	9.24	.09	210	8.10	.08
105	8.36	.10	245	8.09	.08

graphically in the figure. In the table also is given the A.D. of each mean (the mean deviation divided by $\sqrt{8}$) which is employed as a precision measure.

The indication of a maximum point on the graph in the interval, 35 to 105 minutes, is statistically significant, and the decline followed by attainment of an almost level plateau is a surprising result. Here, too, the precision measures indicate that this is not due to mere chance.

ROBERT TENNANT, JR.
DEPARTMENT OF PATHOLOGY,
YALE UNIVERSITY

THE EFFECT OF SUNLIGHT ON HUMAN BLOOD CELLS

EXPERIMENTS were made to elucidate the action of sunlight on human cells under the condition of general insolation. The first experiments concerned the cells which are easiest to obtain, namely, the red blood corpuscles. The results of these experiments are described below.

It was found first that, in accord with the results obtained by Pfeiffer and Bayer, Hausman and Loewy, and Löhner, red corpuscles suspended in a salt solution exposed to direct sunlight gradually disintegrate and hemolize. This disintegration occurs, however, only if the corpuscles are illuminated by intensive sunlight (clear sky, dry air, at noon). Under such condition the first hemolysis is noticed 10 minutes after beginning of insolation. Diffuse sunlight does not produce any hemolysis. Further experiments show, however, that even diffuse light which is fifty times as weak as direct sunlight makes the corpuscles less resistant and accelerates their spontaneous hemolysis when after the illumination they are kept in the dark in a physiological salt solution. After the exposure of the suspension of corpuscles to direct sunlight their resistance against poisons and hypotony is markedly diminished. But among the rays of sunlight only visible rays decrease this resistance. If the corpuscles are exposed to direct sunlight in quartz tubes no decrease or even an increase of the resistance is observed. Special experiments showed that ultra-violet rays, if they are weak or act for a short time, increase the resistance of the corpuscles and protect them against the harmful effect of visible rays. However, strong ultra-violet rays from a mercury vapor lamp destroy the red corpuscles.

In *in vivo* experiments it was found that a total insolation of men lasting 10 minutes (December, January, in Arizona) increases the resistance of young red corpuscles and decreases the resistance of old corpuscles, the latter being more sensitive to visible light. The insolation of the same men for one hour increases the resistances of all their corpuscles very markedly, but only if the sunlight contains a sufficient amount of ultra-violet rays (clear sky). This increase of the resistance is not lasting and disappears within twenty-four hours. When the sky is partially covered by clouds and the humidity is high or when sunlight is filtered through glass plates (6 mm thick) a marked decrease of the resistance of all corpuscles is observed, and this decrease does not always disappear within twenty-four hours.

The author's experiments twenty years ago showed that the permeability of protoplasm of plant cells for water soluble substances is greater in light than in the dark. This observation was confirmed later by many scientists not only on plant but also on animal cells. We might expect, therefore, the same action of light on red corpuscles. Indeed the present experiments show that the permeability of red corpuscles for water soluble substances is increased by sunlight, and this effect is due to the visible light in this case, too. Human red corpuscles were investigated in respect of their permeability for grape sugar. It may, therefore, be assumed that the nutrition of red cells is enhanced by the action of sunlight. As the chemical and physical structure of red corpuscles is similar to that of colorless cells, the results obtained on red corpuscles can be extended to all cells of our organism, and it is likely that sunlight increases the nutrition of our organism in general.

W. W. LEPESCHKIN
FROM THE LABORATORIES OF THE
DESERT-SANATORIUM AND
INSTITUTE OF RESEARCH,
TUCSON, ARIZONA

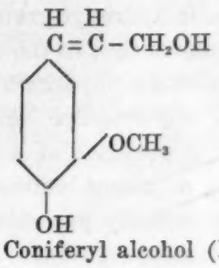
THE ISOLATION OF NORMAL PROPYL GUAIACOL AS A DEGRADATION PRODUCT OF LIGNIN

THE woody portions of plants, such as cobs, hulls, stalks, leaves, trunks of trees and shrubs, are composed

principally of carbohydrates, mostly cellulose and pentosans, and a complex designated as lignin. The latter is an amorphous substance ranging in color from light brown to black, depending on the method used in its isolation.

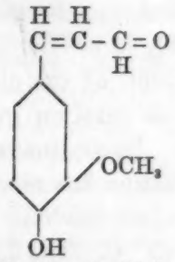
Commercially, lignin is obtained as a by-product in the preparation of paper pulp from wood. When wood is digested either with a calcium bisulphite solution or sodium hydroxide or sodium hydroxide and sodium sulfide solutions, under pressure, the lignin is dissolved, leaving the cellulose in a more or less pure state. The lignin obtained in this operation is generally discarded, and its disposal aggravates seriously the river pollution problem. When one considers that in this country alone approximately 1,500,000 tons of lignin are annually discharged from the various pulpwood mills, the wastefulness of this operation will be readily realized. Furthermore, there is produced annually on the farms of this country millions of tons of various agricultural waste materials, such as cornstalks, corn-cobs, cereal straws, flax straw and hulls, 10 to 20 per cent. of which is lignin. The fact that lignin now constitutes an enormous industrial and agricultural waste material has stimulated great research activity, on the fundamental chemistry and the economic utilization of this natural product.

Although lignin has been studied by chemists for nearly a century, the chemistry of this substance is still obscure. Considerable difference of opinion prevails among investigators as to whether this substance belongs to the aliphatic, aromatic, hydroaromatic or heterocyclic series. Among the advocates of an aromatic structure for lignin may be mentioned Klason,[1] Kürschner[2] and Freudenberg[3] and coworkers. On the other hand, Willstätter and Kalb,[4] Jonas[5] and Marcusson[6] contend that lignin is either hydroaromatic or belongs to the heterocyclic series. Klason has modified his conception of the structure of lignin from time to time, but he has generally assumed that it is structurally related to coniferyl alcohol (I)



Coniferyl alcohol (I),

or coniferyl aldehyde (II)



Coniferyl aldehyde (II).

Kürschner assumes that lignin is a polymer of the glucoside coniferin, which Tiemann and Haarmann[7] have shown yields glucose and coniferyl alcohol upon hydrolysis. The fact that lignin does not behave like a true unsaturated compound would speak against the Klason and Kürschner conception of the structure of lignin. Further, when lignin is heated with hydriodic acid as in the Zeisel determination of alkoxyl groups, methyl iodide only is given off. This has been definitely established in the case of spruce[8] and oat-hull[9] lignin. If lignin were structurally related to coniferyl alcohol or a derivative of coniferyl alcohol or were a polymer of coniferin, one ought to obtain a mixture of methyl iodide and ethyl iodide upon heating it with hydriodic acid. Tiemann and Haarmann[10] have shown that coniferyl alcohol when heated with hydriodic acid gives both methyl iodide and ethyl iodide, the latter resulting from a degradation of the pro-

[1] P. Klason, Svensk Kem. Tidskrift, 1897, p. 135; "Beitrag zur Kenntnis des chemischen Baues des Tannenholzlignins," *Chem. Zentralblatt*, 90, I, 92 (1919); "Über Lignin und Ligninreaktionen," *Ber. deut. chem. Ges.*, 53, 706 (1920); 'Beitrag zur Kenntnis der Konstitution des Fichtenholzlignins," *Ibid.*, 53, 1864 (1920); "Beitrag zur Konstitution des Fichtenholz-Lignins," *Ibid.*, 58, 375, 1761 (1925); *Ibid.*, 62, 2523 (1929); *Ibid.*, 63, 792, 1548, 1983 (1930).

[2] K. Kürschner, "Zur Chemie der Ligninköper," p. 141, published in F. B. Ahrens' "Sammlung Chemischer und Chemischtechnischer Vorträge," 28, Ferd. Enke, Stuttgart, 1926.

[3] K. Freudenberg, "Zur Kenntnis des Fichtenholz-Lignins," Sitzungsberichte d. Heidelberger akad. d. Wissenschaften (*Math.-Naturwissenschaft*), 1928, Abhandlung, 19; K. Freudenberg, H. Zocher and W. Dürr, "Weitere Versuche mit Lignin," *Ber. deut. chem. Ges.*, 62, 1814 (1929).

[4] R. Willstätter and L. Kalb, "Über die Reduktion von Lignin und von kohlenhydraten mit Jodwasserstoffsaure und Phosphor," *Ber. deut. chem. Ges.*, 55, 2637 (1922).

[5] K. G. Jonas, "Das Problem des Lignins," Wochenbl. f. Papierfabrikation 56, No. 24*A*, 83 (1925).

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[7] F. Tiemann and W. Haarmann, "Über das coniferin und seine Unwandlung in das aromatische Princip. der Vanille," *Ber. deut. Chem. Ges.*, 7, 608 (1874); F. Tiemann, "Zur Kenntnis des Glieder der Protocatechureihe," *Ber. deut. Chem. Ges.*, 11, 659 (1878).

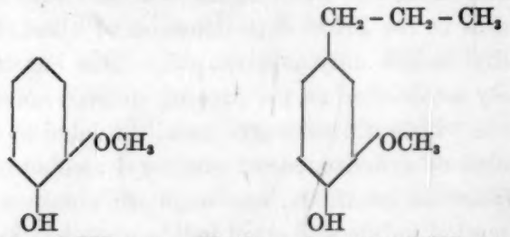
[8] E. Hägglund and B. Sundroos, "Zur Kenntniss Alkoxylgruppen des Holzes und des Lignins von Fichte," *Biochem. Zeitsch.*, 146, 221 (1924).

[9] M. Phillips, "The Chemistry of Lignin. IV. Lignin from Cat Hulls," *J. Am. Chem. Soc.*, 52, 793 (1930).

[10] Ref. 7.

pylene side chain. The conception of Freudenberg[11] that lignin is made up of vanilyl and piperonyl components, the union taking place through the hydroxyl group of the vanilyl constituent, although more in harmony with the experimental facts, is still open to objection in that the assumption is made that there are no free phenolic hydroxyl groups in lignin. Klason[12] has recently shown that in the case of lignin sulphonic acid, free phenolic hydroxyl groups are present. Furthermore, the evidence offered by Freudenberg[13] in favor of the presence of a methylene oxide group in lignin is by no means convincing.

In a paper recently presented by the writer before the Cincinnati meeting of the American Chemical Society, it was reported that when lignin was distilled with zinc dust at 400° C. in an atmosphere of hydrogen, an oil was obtained which amounted to 16 per cent. of the weight of the lignin used. Approximately, 55 per cent. of the oil was phenolic in character, and in this fraction guaiacol (III) was definitely identified. In continuing this line of investigation, another fraction has now been isolated from the phenolic portion of the oil. This fraction distilled over at 203 to 215° C. The 3,5-dinitrobenzoyl derivative of this was prepared, and after repeated crystallizations from ligroin and 95 per cent. ethanol, a crystalline substance, was obtained which melted sharply at 116.8° C. (cor.). This melting point corresponded exactly to that of the 3,5-dinitrobenzoyl derivative prepared from a pure specimen of normal propyl guaiacol (IV) (1,N. Propyl-3-methoxy-4-hydroxybenzene). When the two compounds were mixed



Guaiacol III N. Propyl Guaiacol IV

the resulting mixture was also found to melt at 116.8° C. (cor.). The identity of these substances was further confirmed by the optical properties[14] of the crystals.

The isolation of guaiacol and n-propyl guaiacol as degradation products of lignin is believed to be of considerable significance from the standpoint of the constitution of lignin. It would appear, in all probability, that the two fundamental units in the structure of lignin are guaiacol and n-propyl guaiacol, the latter having the hydrogen atoms in the n-propyl side chain substituted by alcoholic hydroxyl groups. Just how the union takes place is, of course, not known, but probably through the n-propyl side chain in such a manner that leaves most of the phenolic hydroxyl groups free.

To account for the well-known reaction of lignin with bisulphites, it is not necessary to assume, as some investigators have done, the presence of an unsaturated bond in this complex, but can be explained according to Fuchs[15] on the basis that the phenolic nuclei may behave in their tautomeric form and, therefore, as unsaturated cyclic ketones.

This conception of the structure of lignin is in complete harmony with all the known facts as to its chemical behavior, and points the way toward a more complete understanding of its complex constitution.

MAX PHILLIPS

CONTRIBUTION FROM THE COLOR AND FARM WASTE DIVISION, BUREAU OF CHEMISTRY AND SOILS, U. S. DEPARTMENT OF AGRICULTURE, WASHINGTON, D. C.

[11] Ref. 3-b.

[12] P. Klason, "Beiträge zur Konstitution des Fichtenholz Lignins," *Ber. deut. chem. Ges.*, 63, 792 (1930).

[13] K. Freudenberg and M. Harder, "Formaldehyd als Spaltsück des Lignins," *Ber. deut. chem. Ges.*, 60, 581 (1927).

[14] The optical properties of the crystals were determined by G. L. Keenan of the Food and Drug Administration of this Department.

[15] W. Fuchs, "Uber Lignin und den Sulfit-Kochprozess," *Ber. Deut. chem. Ges.*, 54, 484 (1921).

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